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THE EPISTEMOLOGICAL STATUS OF THE CHEMICAL CONCEPT OF ELEMENT (I)*

F. A. PANETH

1 *The Need for Epistemological Clarification of the Fundamental Concepts of Chemistry*

DISCUSSIONS of the principal concepts of chemistry are few and superficial, in striking contrast to the many and penetrating investigations into the philosophic foundations of physical theories. In previous centuries there was an obvious reason for this: many physical doctrines were already common knowledge amongst the educated, whilst chemistry, in the scientific sense, did not exist at all, or was only just beginning. But even today we find a wider spread of physical than of chemical knowledge and interest amongst philosophers, whether this be due to tradition in the profession or to the curricula of our secondary schools. The first two laws of thermodynamics, the questions concerning action at a distance and contact action, the difficulties of the ether hypothesis, and more recently, particularly the principle of relativity and the quantum theory, are all problems familiar to every philosopher who wishes to take account of the results of the exact sciences in his system. On the other hand, one rarely finds a discussion of a specifically chemical theory in the writings of a philosopher. The atomic theory is an exception. This, however, belongs just as much to physics as to chemistry, and indeed the physical aspect (divisibility, mutual attraction and repulsion, and so on) has been discussed much more than the chemical (qualitative characteristics, valency, etc.). As a rule, chemistry is presented by the philosophers as a science which is well on the way to transforming itself into

* The text of a public lecture given by Professor F. A. Paneth to the Gelehrte ellschaft of Königsberg in 1931. (German text published in *Schriften der gsberger Gelehrten Gesellschaft*, Naturwissenschaftliche Klasse, 1931, Heft 4 [Max eyer Verlag].) Translated by H. R. Paneth.

physics, and to which, therefore, the same considerations will apply in due course.[1]

Not only philosophers, but many physicists have endeavoured to clarify the epistemological foundations of physics, sometimes in systematic investigations, sometimes merely by way of occasional (but extremely illuminating) comments. I need only recall in this connection the names Helmholtz, Kirchhoff, Heinrich Hertz, Mach, Boltzmann, Poincaré, Planck, Einstein, Weyl, Jeans, Eddington, Bohr, and Heisenberg. For this reason there has long been an extremely fertile interaction between writings in philosophy and in physics, while we have hardly anything comparable in the field of chemistry.

The fact that there has been little thorough philosophical work on the concepts of chemistry is particularly apparent in the case of the concept most fundamental to the whole science: the concept of 'element'. Most textbooks of chemistry, when introducing the concept of element, make a few remarks about its philosophic formulation by Aristotle, and in the Middle Ages, and emphasise the difference from the present-day scientific idea of element. With stereotyped uniformity the statement is repeated that the four elements of Aristotle did not designate substances, but combinations of the fundamental properties cold, warm, dry, wet; that Boyle was the first to mean by a chemical element a substance that could not be decomposed further, etc. In most expositions of the history of chemistry, too, we find similar statements.[2] There is no doubt, however, that it is

[1] Exceptions to this rule are the writings of H. Rickert (particularly *Die Grenzen der naturwissenschaftlichen Begriffsbildung*, Tübingen, 1921); Émile Meyerson (particularly *Identité et Réalité*, Paris, 1908, and *De l'Explication dans les Sciences*, Paris, 1921); and E. A. Burtt, *The Metaphysical Foundations of Modern Physical Science*, London, 1925.

[2] To name but a few of the best-known works: E.v. Meyer, *Geschichte der Chemie*, Leipzig, 1905, p. 7 (English translation, 3rd edition, 1906); W. Ostwald, *Der Werdegang einer Wissenschaft*, Leipzig, 1908, pp. 5, 9; W. Ramsay, *Essays Biographical and Chemical*, 1908, pp. 7, 25; S. Arrhenius, *Theorien der Chemie*, Leipzig, 1909, pp. 11, 14; F. Soddy, *Matter and Energy*, London (no date given: *circa* 1912), p. 39; W. Herz, *Grundzüge der Geschichte der Chemie*, Stuttgart, 1916, pp. 9, 35; F. Dannemann, *Grundzüge einer Geschichte der Naturwissenschaften*, Leipzig, 1903, II, p. 188; A. Sieverts in the *Handwörterbuch der Naturwissenschaften*, Jena, 1912, II, p. 438.

This questionable presentation of Aristotle's doctrines can probably be traced back to H. Kopp's *Geschichte der Chemie* (Braunschweig, 1843, I, p. 30). We must add, however, that Kopp himself later presented a perfectly correct formulation in the 'Beiträge zur Geschichte der Chemie' (*Ansichten über die Aufgabe der Chemie*, Braunschweig, 1875, p. 5). Unfortunately little notice has been taken of this version.

incorrect to characterise the elements of Aristotle as properties rather than substances, and that Boyle's innovation lies in a different direction from the assumption that the elements are, by their nature, substances and that they cannot be decomposed. The fact that there is nevertheless general agreement with these statements amongst the majority of chemists writing on this subject only shows how little importance they attach, in general, to the investigation of the concepts used in chemical teaching.

The study of the philosophic-historical development of the concept of element, in particular, is interesting and important for a further reason. The same epistemological questions which were raised in connection with it in classical times and were even in part answered, still play an important part in present-day chemistry. I am primarily referring here to the question 'In what sense may one assume that the elements persist in compounds?' When one compares the obscure remarks of some modern authors on this subject with the insight already gained by philosophers of antiquity, one becomes convinced that in chemistry, just as in physics, a philosophic clarification of the fundamental concepts should promote scientific understanding as well. There is the further point that we may, by such an investigation, test whether or not it is true that chemistry should and will dissolve into physics.

For these reasons we shall try in this essay to present the epistemological status of the concept of element somewhat more thoroughly than is customary in chemical textbooks. To make communication possible it is necessary to use certain technical philosophical terms; their choice will, of course, vary with one's basic philosophical conceptions. Since, however, we are here only concerned with the discussion of a particular problem pertaining to science—which must retain its meaning for every epistemological position—it is not of decisive importance which terminology we use. For reasons of expediency we shall follow in the main the terminology chosen by Eduard von Hartmann, since, amongst the theories put forward and developed in detail by professional philosophers, his epistemology possibly comes closest to an understanding of the essential nature of science.[1] We can ignore here the question whether Hartmann's

[1] Cf. E. v. Hartmann, *Kritische Grundlegung des transzendentalen Realismus*, 1875; *Das Grundproblem der Erkenntnistheorie*, 1889; *Grundriss der Erkenntnislehre*, Bad Sachsa, 1907; *Grundriss der Naturphilosophie*, Bad Sachsa, 1907.
I wish to emphasise particularly that, like Hartmann, I am using the word

' transcendental realism ' in some points even depended too much on the science of his time, just as we can, for our purpose, completely ignore his metaphysical system.

2 *The Concept of Substance in Chemistry*

Chemistry, like every natural science, started from the naive-realistic world-view, and gradually found itself compelled to apply corrections to this. It is, however, characteristic of chemistry that it has not advanced as far in the application of these corrections as some other sciences; indeed it is of the essence of its fundamental concepts that they have retained quite an appreciable ' naive-realistic residue '.

Let us consider first the concept of substance in this respect. After all, the whole doctrinal structure of chemistry rests on this. Moreover, it is fundamental to the concept of element, with which we are concerned here, in particular. It is well known that it has long been the aim of physics to reduce sensory qualities to quantitative determinations.[1] Nature is assumed to be without qualities, and the properties of colour, sound, taste, smell, etc., are applicable only to our sense representations. The objective prerequisites of the last two qualities are still almost totally obscure; but since even today, as formerly, these qualities play an important part in chemistry, in characterising substances, the chemist has no choice but to assign the properties of taste and smell to the substances themselves, in completely naive-realistic fashion. Nobody objects, in fact, to speaking of the salty taste of sodium chloride, or the unpleasant smell of hydrogen sulphide. Here we find ourselves, as is surely unnecessary to elaborate, still standing with both feet on the ground of naive realism, without, it should be emphasised, being aware that this lack of philosophic clarity entails any disadvantage. Indeed, even in the case of that ' property of substances ' which can be reduced to quantitative deter-

' transcendental ' in its epistemological sense only, i.e. meaning ' beyond the sphere of consciousness '.

[1] Even epistemologists who deny the actual existence of a world devoid of qualities allow this as an epistemological principle of physics; see, for instance, H. Rickert, ' Psychophysische Causalität und psychophysischer Parallelismus ' (in *Sigwart-Festschrift*, Tübingen, 1900), p. 75.

M. Schlick (*Allgemeine Erkenntnislehre*, Berlin, 1918, pp. 214, 243), in particular, has emphasised that the exclusion of qualities can only be complete with respect to the sensible qualities.

mination most easily—colour—we usually refrain, for the purpose of chemical characterisation, from so reducing it: cinnabar 'is' red, gold 'is' lustrous. While the introduction of the various constants would make a more exact numerical statement possible in such cases, this would usually be too cumbersome. After all, we find something similar in physics, where we do not mind speaking of red and green light as rough data; quality is retained here as 'useful mental shorthand'.[1]

When one attacks the problem of substance more closely with the weapons of epistemology, the fact is revealed (as has already been pointed out by many authors) that not only may none of its sensory properties be taken to exist in nature, but substance disappears altogether. The old view, taken by the Greek atomists, that only size, shape and motion are real, while colour, smell, etc., are mere appearances (a view revived later by Locke in his distinction of primary and secondary qualities), marks but an arbitrary stopping point in this process of dissolution, though (as we shall discuss below) a very useful one for the sciences. The subjective nature of the primary qualities, still pertaining, according to this view, to the things themselves, has been demonstrated in the well-known idealistic lines of thought (Berkeley, Hume, Kant). Exponents of 'transcendental realism', too, who are convinced of the existence of an external nature which is knowable to a certain extent, find themselves forced, in pursuing the logical consequences of atomism to the end, to declare matter to be "not something real, but merely an appearance" which is produced by the joint effect of closely adjacent forces "in and for the conscious mind".[2]

Now it is particularly interesting that, independently of these ilosophic considerations, progress in theoretical physics, also, has ecessarily to the complete dissolution of the concept of substance. f the classical kinetic theory of matter is long past; molecules are no longer solid spheres; the space assigned to them y electric forces originating in the positive protons and s, which are vanishingly small compared with the ow one might regard protons and electrons as last

rundriss der Naturphilosophie, Bad Sachsa, 1907, p. 23.
s der Erkenntnislehre, Bad Sachsa, 1907, p. 73. Similarly Bad Sachsa, 1907, p. 105. Cf. also W. Wundt, *Logik*,
ewhere the concept of matter reduces to moving

remnants of the old concept of substance, if modern quantum mechanics did not require the assumption that the motions of these particles do not take place in the three-dimensional space which we can picture, but in spaces of many more dimensions. Thus we have lost the last possibility of retaining, in any sense at all, the familiar notion of substance; we renounce all possibility of visualising, while raising calculability to a level never before attained. Here physics is harvesting the fruit of a tree planted centuries ago. Starting with Galileo, more and ever more weight was attached to mathematical formulation; the successes of this methodology provided encouragement for further and further advances along the same route, and we must not be surprised when present-day physicists, rather like the ancient Pythagoreans, see the true nature of the world in mathematical relations.[1]

Chemistry did not follow this course of development. Indeed, already in the seventeenth century, at the time of the first beginnings of scientific chemistry, we find one man who, with the greatest possible clarity, demanded for it principles different from the mathematising ones of physics: the Englishman, Robert Boyle. Boyle is one of the few scientists with a mental range wide enough to master the procedure adopted in physics, and appreciate it on its merits, without thereby according it the status of the only true path to salvation in natural science. On occasion he states quite definitely that the secondary qualities are no less real than the primary ones,[2] and that it is not necessary to give up the former and calculate only with the latter to obtain scientific results, and find thereby methods for the practical mastery of nature.[3]

Nevertheless, he himself always considered mathematical formulation to be a higher level of theoretical insight into nature.[4] His

[1] See, for instance, Sir James Jeans, *The Mysterious Universe* (Cambridge, ... For a survey of the changes in the concept of matter in physics see the ess... Weyl, *Naturwissenschaften*, 1924, 12, 561, 585, 604.

[2] Robert Boyle, 'The Origin of Forms and Qualities' (*The Works ... Robert Boyle*, Birch Edition, London, 1772, III), pp. 22, sqq.

[3] 'Certain Physiological Essays. A Proemial Essay' (*The W...*

[4] Ibid. p. 308. "I consider then, that generally speaking ... an effect or phaenomenon, is to deduce it from something ... than itself; and that consequently there may be divers ki... of the same thing. For although such explications bo... understanding, wherein it is shewn, how the effect is pr... and catholick affections of matter, namely, bulk... these explications to be despised, wherein...

successors amongst the chemists, while fol
practical chemist, abandoned his penetratin
completely neglected the classical, pure, qu
which he had particularly fostered; instead,
reservation purely qualitative hypotheses,
intolerable from, say, the more severe pc
These hypotheses were often very useful
chemistry. We need only remember the i
'phlogiston'[1] at the beginning of the eig
force'[2] at the beginning of the nineteenth c

On the other hand, the Newtonians' i
such tendencies emerges particularly from tl
Kant that "in any particular discipline of th
find only as much actual science as there
accepts this definition, Kant is perfectly righ
amongst the sciences, since chemistry is esse
he saw this more clearly than later philosopl
istry to pass into physics. He did, howeve
'systematic art'.[3] Chemists may be satisfi
subject, if, indeed, they must remain excl
genuine 'science'. However, Kant's definit
narrow, and therefore probably inappropri
A closer consideration of chemistry—not t
and historical sciences—suffices to show t
particularly so-called physical chemistry, a
much mathematics that they would presum
tion; but there also exist analytic and sy
have remained completely unmathematical t
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rsal ones formerly named. For in
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ss, that the nearer the discove
eries of causes, the more is

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unhampered by mathematics or indeed almost any theory, who discovered the majority of all chemical elements on the basis of the most primitive concept of substance! Much later, when a theory of chemical elements existed, and physicists realised that the elements must follow each other as the ordinal numbers, and that their number is therefore strictly limited, they were astonished to find that the analysts had already discovered almost all possible elements. A ' systematic art ' that can achieve this may surely be called a science as well. Moreover, already in the seventies of the last century, the elements had been arranged by the chemists into a scheme, the so-called ' natural system of the elements '. Present-day workers on the theory of the atom have contributed enormously to the deeper understanding of this natural system; but the arrangement itself has not undergone any essential change in the last sixty years. Perhaps even more imposing is the achievement of the organic chemists, who have been able to bring hundreds of thousands of different chemical substances into a clear system on the basis of a very few hypotheses concerning the combination of atoms. Here, too, the methods of research used in physics are already being applied and much splendid progress has been made in this way; but much more astonishing than individual corrections, achieved with the most modern aids, is the quite overwhelming number of cases where the organic chemist's simple concept of valency, with its obvious weaknesses, has made possible completely correct statements about the mutual linkages of atoms. Even if the character of chemistry should change essentially in the future owing to penetration by mathematico-physical methods, its history during the nineteenth century, in which it achieved such successes without mathematics, must never be ignored in its philosophic evaluation.

In view of all this, we declare chemistry, too, to be a true sci[ence] even in those branches where it contains little or no math[ematics]. Indeed, perhaps it is just there that we are dealing with ch[emistry] the strictest sense of the word. Emil Fischer, in his au[tobiography] tells of a colleague at Würzburg who had the char[acterisation] ' Chemistry is bangs and stinks '.[1] We can essentiall[y accept] and interpret it as follows: chemistry is a scienc[e] directed towards the secondary qualities of sub[stances].

[1] Emil Fischer, *Aus meinem Leben*, Berlin, 1922, [p.] 1931, 127, 526): " The schoolboy's name for chem[istry] and a very appropriate name it is."

Taking this philosophically primitive standpoint chemists have usually managed very well in the whole vast range of the subjects of analysis and synthesis. For this reason they have remained unaware that, after all, somewhat deeper epistemological consideration is necessary for the complete understanding of the reactions they have carried out and the theorems they have put forward. This necessity will, however, at once become apparent when we turn to the concept of chemical element. Here we are bound to get involved in irresolvable contradictions if we retain the chemist's naive notion of substance.

3 *The Epistemological Standpoint of the Ancient Atomists*

It is probably useful to preface the discussion of the modern concept of element with a few remarks about its predecessors in classical and medieval times. We do this, not with the intention of presenting an exhaustive history of its development, but because we find the various views very well represented; instead of abstract arguments we can frequently make the intuitively comprehensible theorems of the ancient philosophers the basis of our discussion.[1]

'How could hair have its origin in non-hair, or flesh in non-flesh?' is the text of one of the surviving fragments of the writings of Anaxagoras.[2] Already in those days, according to Aristotle's testimony, all physicists were agreed that matter is permanent.[3] Anaxagoras went further than some of his contemporaries and predecessors only in considering qualitative, as well as quantitative invariance a matter of course. In his question the basic problem of chemistry and its conceptual difficulty are pointedly presented. When he raised this question there were already some typical attempts to solve the problem. These we will consider briefly, since they lead almost directly to the [e]pistemological problem in which we are interested.

[T]he conviction that the truly existent could not perish had led the [E]leatics to declare all changes in the world of the senses to be mere

[1] We shall disregard here the deliberations of Wald and Ostwald, who deviate [f]rom the definition of element and follow their own anti-atomistic lines of thought. We shall disregard all remarks (for instance, those of Fritz Mauthner in his [G]eschichte, Munich, 1910) that remain on the philological level and fail [t]o penetrate the content of the concept.

[2] Diels, *Fragmente der Vorsokratiker*, Berlin, 1906, I, p. 317, Fragment 10 [a]nd Fragment 8 of Empedocles (H. Diels, op. cit. p. 175): "There [i]s no coming-to-be, nor any end in accursed death. Rather there is only [t]hings mingled. Birth is only a name given to this by

appearance. Conversely, Heraclitus was led, by contemplation of the continual change in nature, to deny any permanence. The former took up a position so much in contradiction to everyday experience that they were exempted, from the start, from the task of explaining nature. For this reason Aristotle refers to them as the opposite of scientists (ἀφυσικοί).[1] But Heraclitus' view does not agree well with experience either, for according to his doctrine one would expect that every substance could change into every other, which even in those days must have been seen to be untrue. An intermediate position is taken up by Empedocles. His doctrine of the four elements constitutes an enormous advance. Indeed, one may justly argue that the assumption of a finite number of permanent substances which, by being mixed in different proportions, produce the multitude of objects we perceive, is an essential part of present-day chemical doctrines.[2] No answer, however, is given to Anaxagoras' question; it seems that it is not even raised by Empedocles. Yet the creation of a new substance by mixing two known ones is at first incomprehensible precisely on the assumption that the substances are permanent. Thus we must rate very highly the logical rigour of Anaxagoras, who rejected this inconsistent idea, and assumed smallest particles of all homogeneous substances to be distributed everywhere in the universe. Indeed, it is worth noting that rather similar objections were still raised much later by philosophers and chemists. Thus John Stuart Mill asserts for this reason that the laws of chemistry " owe their existence to a breach of the principle of Composition of Causes ";[3] while Berthelot considers the difference between the properties of common salt and those of its elements, sodium and chlorine, so incomprehensibly great that he feels bound to refute at length the notion that common salt contains a third element, with less divergent properties, as well.[4] Fund mentally this difficulty could be overcome only by adopting a r profound epistemological standpoint, an achievement which w consider one of the greatest merits of the atomists.

[1] Sextus Empiricus, *Adv. Mathem.* X, 46 (see H. Diels, op. cit. p

[2] Th. Gomperz, *Griechische Denker*, Leipzig, 1911, p. 185. do not agree with the view of W. Windelband (*Lehrbuch der* Tübingen, 1907, p. 34) who sees in the homoiomeries of A of the chemical elements. Precisely what Anaxagoras completely homogeneous new substances from others d istic of chemical doctrines.

[3] J. S. Mill, *A System of Logic*, Book III, Chapter

[4] Cf. E. Meyerson, *Identité et Réalité*, Paris, 19

The atomists were the first to realise that one may indeed retain the demand of the *logos* for a permanent substratum, but not with reference to our world of appearances; or, to put the same idea differently, that laws of nature may be constructed not for our subjective perceptions but only for an objective world quite different in kind, which lies behind those perceptions. Thus they were not forced either to deny the changes in the world of appearances, as did the Eleatics, or to deny permanent substance, as did Heraclitus. What looks to us like coming into being and ceasing to be is actually only re-arrangement of the permanent atoms. The secondary qualities exist only in our perceptions, the primary ones pertain to the atoms themselves; in their realm only size, shape, and motion exist. Thus, 'the conceptual view of the world' demanded by the Eleatics, was 'brought into harmony with appearances'.[1]

The question 'how can hair come from non-hair, and flesh from non-flesh?' is now answered to this effect: the same particles which in one particular arrangement give rise to our impression of green meadow plants, serve in another arrangement for the construction of the animal body fed on these plants, and appear to us to be hair and flesh. In this way the demand for permanence of substance is satisfied with no necessity for Anaxagoras' assumption, viz. that particles of flesh and hair already exist in the plant. Empedocles' happy idea of replacing generation and decay by mixing and separation also plays its part for the atomists; but while with Empedocles it is still impossible to imagine how the mixture of indestructible elements could take place, atomism provides a picture which is both useful and intuitively very acceptable, by assuming smallest particles which juxtapose themselves in different arrangements.[2] It may not be superfluous to show ...t on this basis, which was constructed by Leucippus and Democritus, ...n atomic world without qualities, it was also possible to arrive ... difficulty at a more detailed idea of the nature of a chemical ...d.

...rtant step in this direction was taken by Epicurus.[3] ...ised the objection that no substance with new proper... ...lt from the juxtaposition of smallest particles, since

...*Problem der Materie in der griechischen Philosophie*, Münster,

...hat Empedocles' doctrines, worked out to their logical ...tomistic ideas (see C. Bäumker, op. cit. p. 71).

... *zur Geschichte des Erkenntnisproblems im Altertum*,

sharp eyes would surely still recognise the known substances in the mixture. Epicurus rightly sought to overcome this difficulty by shifting to the realm devoid of qualities, although in developing this idea he did not quite arrive at our present standpoint. In contrast to the founders of the atomistic doctrine, he attributed objectively real sensible qualities to substances: he denied them only to atoms. Now if the properties of substances only arise as a consequence of the coming together of many atoms of the same kind, it becomes understandable why in the case of dispersion into atoms, as effected by mixing, the properties of the previously existing substances disappear and new ones arise from the joint action of the mingled atoms. From this we learn how valuable an instrument the atomic theory was, long before Dalton, in giving an intuitive explanation of chemical processes. The later development by Boyle and Lemery of more definite ideas about the cohesion of smallest particles on entering into chemical combination, and the attempt by present-day atomic theories to comprehend the change in electronic configuration of atoms when coming together, are only more detailed developments of the old lines of thought.[1]

In principle Epicurus would have already reached the present standpoint, had he not considered the qualities of substances to be objectively real. Epistemologically this constitutes a step backwards from Democritus. But this was of no practical consequence, since Epicurus did not, after all, ascribe qualities to the atoms, but only to the substances formed by them; in other words, he only made the small epistemological simplification (retaining some of the naive-realistic convictions) which we mentioned initially as being thoroughly appropriate and justified in chemistry. Since he was not guilty of this error in his atomism, he was able to explain changes of properti occasioned by chemical combination, in the way we have sketc

[1] In more recent days Lemery's extreme ideas of atoms with points v off and remain stuck in other atoms (see, for instance, his explanatio of aqua regia in his *Cours de Chymie*) have been censured sharply atomism. This criticism appears less reasonable when we c analogy with the most modern theories, in which we speak o far, outer orbits of strongly reactive atoms, and assume th ineffective by entering gaps in the electron rings of oth compound is formed (cf. W. Kossel, *Ann. d. Phys.* 191

Boyle's view on the cause of the change in proper compound is expressed in the second part of the *Sc* pp. 458, 506 sqq.).

without having to assume changes in qualities as part of the nature of things. Aristotle, the opponent of atomism, did not shy away from this consequence (a change in qualities) which could be well explained in terms of his philosophic principles. We shall discuss presently the contradictions which arise if one follows him, as many modern chemists do, in not distinguishing the world of the transcendental from the world of appearances, but speaks nevertheless of a persistence of the elements in compounds.

Epicurus' denial (in common with the old atomists) of qualities to atoms, and his admission of only one homogeneous primodial matter does not, in our view, constitute an essential difference from the so-called ' qualitative atomism' of present-day chemistry. An approach favoured particularly, but not exclusively, in popular expositions is exemplified by the formulation that the atoms of gold are gold still, while the properties of a compound are retained in its individual molecules, but lost on decomposition into atoms. This version, which is designed to make the difference between elements and compounds intuitively understandable, is, after all, no more than a very inaccurate formulation. Epicurus would rightly have objected that only the coming together of many atoms generates the qualities, and, in fact, no chemist would seriously maintain nowadays that any property of gold could be discerned in the individual atom. Not only today when the electronic theory has already successfully begun to elucidate the structure of atoms, does the notion of a single primary substance seem quite modern; for philosophic reasons ' qualitative atomism ', too, could never ascribe qualities in the strict sense to individual atoms. We must, therefore, regard the conception of a primary substance as a necessary consequence of their correct epistemological standpoint also in the case of the atomists,[1] in contradistinction to the preceding thinkers. In the case of the latter it was presumably only a rash generalisation or, if one agrees with Joel's opinion, hows the genesis of nature philosophy in the spirit of mysticism '.[2]

s a result of these observations we affirm that some Greek thinkers dy realised that it is the aim of the natural sciences to find the

ge from Theophrastus in Diels, op. cit. I, 371, 42 (cf. Bäumker, op. arkable that many present-day chemists have lacked this epistemo- ve seriously maintained that the " properties of the elements pletely even in the atoms ". Thus the " disappearance of an atom ", when it undergoes chemical combination, dict the basic assumptions of atomism (I. W. Hinrichsen, tand der Valenzlehre ', *Ahrens' Sammlung chemischer*

ung der Naturphilosophie aus dem Geist der Mystik cation of this idea.

laws of a world that is objectively real, whose changes are indicated in our consciousness by processes quite different in kind; and that to understand the change of properties of substances we require transcendental hypotheses. As the Greeks expressed the antithesis: the φαινόμενον has to be explained by means of the ἄδηλον.[1]

After these preparatory historical remarks we will attempt to determine the philosophic character of the modern concept of element.

(to be continued)

[1] Cf. C. Bäumker, op. cit. p. 313

ACHILLES, THE TORTOISE, AND EXPLANATION IN SCIENCE AND HISTORY*

W. W. Bartley III

> Definition. 1. *Plain Superficiality* is the character of a speech, in which any two points being taken, the speaker is found to lie wholly with regard to those two points.—Lewis Carroll.[1]

Someday, someone will have to give an historical explanation of the widespread interest philosophers and methodologists have shown in the problem of historical explanation during the past two decades. In what follows, I shall restrict myself to the main object—or, perhaps, target—of this interest: the hypothetico-deductive schema or 'model' of causal explanation presented by Karl R. Popper.[2]

I say 'target' because the criticisms of the opinion that this schema applies to historical explanation, as well as to explanation in the sciences, have seemed to grow more barbed almost every quarter.[3] Since

* Received 29. vi. 61.

[1] Lewis Carroll, *The Dynamics of a Parti-cle* Oxford, 1874, p. 63.

[2] This schema was first stated by Popper in his *Logik der Forschung*, Vienna, 1934, in English translation as *The Logic of Scientific Discovery*, London, 1959, especially section 12; and later in his 'The Poverty of Historicism', *Economica*, 1944, 1945, **11, 12**, reprinted in book form, London, 1957; and *The Open Society and Its Enemies*, London, 1945, Chapter 25. His most detailed discussion, to appear in his forthcoming *Postscript to the Logic of Scientific Discovery*, chapter 1, section 15, has appeared as an article, 'The Aim of Science', *Ratio*, 1957, **1**, 24-35. Other important statements of the schema appear in C. G. Hempel, 'The Function of General Laws in History', *Journal of Philosophy*, 1942, **39**, *i.a.*; Feigl and Sellars, Eds., *Readings in* [illegible]*losophical Analysis*, New York, 1949, pp. 459-471; Morton White, 'Historical Ex-[illegible]tion', *Mind*, 1943, **52**; J. W. N. Watkins, 'Ideal Types and Historical Explana-[illegible]*his Journal*, 1952, **3**, and J. O. Wisdom, *Foundations of Inference in Natural* [illegible]don, 1952, chapter 6. For further bibliographical references see 'Ex-[illegible]he subject index of J. C. Rule, Ed., *A Bibliography of Works in the* [illegible] 1945-57, *History and Theory*, (*Beiheft* I, 1961), p. 75.

[illegible]blications contain some of the more interesting criticisms of [illegible]diner, *The Nature of Historical Explanation*, London, 1952; [illegible]ry Narrative in History', *Philosophical Quarterly*, 1954, **4**, [illegible]*History*, London, 1957; Arthur C. Danto, 'On Explana-[illegible]*of Science*, 1956, **23**; E. A. Gellner, 'Explanations in [illegible]*ristotelian Society*, Supplementary Volume 30, 1956; [illegible]History', *Mind*, 1957, **66**; T. A. Goudge, 'Causal [illegible]*his Journal*, 1958, **9**; Peter Winch, *The Idea of a*

these criticisms are mistaken, I shall try to clarify the situation in the following way. First (1 and 2) I will restate the schema and comment on its interpretation—something that has often been neglected in recent debate. Then (3) I shall briefly examine several incorrect arguments against it. Finally, (4) I shall discuss an important error about inference which seems to lie at the root of much of the controversy: the interpretation Gilbert Ryle and others give to Lewis Carroll's story about Achilles and the Tortoise.

Before beginning this discussion, a more general prefatory comment about the controversy is needed. Although the schema's critics have attempted in practice to show that there is at least one important explanatory activity in history writing which cannot be accounted for by the hypothetico-deductive schema, their aims have been much broader. Usually they have also wished to show that historical explanation is quite different in logical structure from explanation in the sciences. Some of the critics have been even more ambitious. Alan Donagan, for instance, concluded his attack with the comment: "These facts do not refute "Scientific Humanism", but they may discourage us from persevering with it'.[1]

There are, however, two rather straightforward requirements that would have to be fulfilled by any such critic of the schema before such discouragement need set in.

(1) The critic would indeed have to make good the claim that history writing involves an important kind of non-hypothetico-deductive explanation. (2) *More important, he would have to show that this different kind of explanation is not also found in the sciences.*

Although the second requirement is clearly essential to the project, the schema's critics seem not to have noticed it. Rather, they appear to assume that to satisfy the first requirement would be to establish *eo ipso* their broader claims. As I shall try to show, they have succeeded even here.

1

Popper's first account of the schema appears a

To give a *causal explanation* of an event means which describes it, using as premises of the

Social Science, London, 1958; Arthur Lee Burns, 'Interna Explanation', *History and Theory*, 1960, 1.

It should be noted that I have classified Gardiner the frequent identification of his 'regularity vie (See final paragraph, section 2 below.)

> *universal laws*, together with certain singular statements, the *initial conditions*. . . . We have thus two different kinds of statement, both of which are necessary ingredients of a complete causal explanation. They are (1) *universal statements, i.e.* hypotheses of the character of natural laws, and (2) *singular statements*, which apply to the specific event in question and which I shall call ' initial conditions '.[1]

The presentation of the schema itself plays a secondary rôle in Popper's discussion; his emphasis is put on several related points: (1) explanation is deductive; (2) conjectures or hypotheses of universal form play a part in all satisfactory explanation, including historical explanation; and (3) scientific activities, whether historical or theoretical, will try to restrict themselves as far as possible to conjectures that are susceptible to test by empirical evidence. Hence, the schema should be understood and discussed in terms of these points and of Popper's more general theory of scientific method.

Had this been done, it is doubtful whether the most common misunderstanding of the schema would ever have arisen. This is the strange idea that the schema is intended as a picture of the *verbal form* a scientific explanation may be expected to take.[2] Clearly, no historian labels and organises his explanations in this fashion; and any attempt to ' reform ' history writing on such lines would be doomed from the start by literary and aesthetic considerations alone. The phrase ' schema of explanation ' refers only to what might be called the ' underlying logical structure ' of the process of explanation. The schema is intended to play the rôle of a sort of criterion or conceptual

[1] K. R. Popper, *The Logic of Scientific Discovery*, pp. 59-60

[2] Dray, op. cit. makes such a mistake. In his ' baseball game ' argument against e schema, he argues—rightly—that we often explain historically not by *referring* to , but by referring to a further fact. A fly ball's being caught twenty feet off the is explained by the fact that the fielder was able to climb up a ladder to catch ems confused, however, about the sort of candidate generalisation which he schema would put forward in such a case. He suggests: ' In base- organised league play—fielders usually catch fly balls.' In offer- —one that is clearly superfluous—Dray is not dealing with the lained ', which is not that a fielder caught a ball per usual, but t when it was twenty feet off the ground. Clearly, when able to catch a ball which was twenty feet off the ground a twenty-foot ladder to catch it, we have assumed in ws, including those of gravity. Once we are clear d in the explanation in question, Dray's insistence ften fielders catch fly balls) would be logically but as irrelevant to his argument.

tool, and to indicate what kinds of assumptions and relations may be challenged when an explanation breaks down.

This interpretation of the schema is not a defensive one made after the fact: it is clearly supported by the illustrations which accompany Popper's presentation of the schema:

> . . . we can say that we have given a causal explanation of the breaking of a certain piece of thread if we have found that the thread has a tensile strength of 1 lb., and that a weight of 2 lbs. was put on it. . . . It is from universal statements in conjunction with initial conditions that we deduce the singular statement, ' This thread will break '.[1]

It is important to notice that although Popper gives this example to *illustrate the schema*, the effect, conditions, and generalisations he actually cites patently do not exhibit the schema's structure. In Popper's simple example it would be comparatively easy to articulate the universal hypotheses and additional conditions which were probably presupposed.[2] In any case, Popper permits stated explanations to depend on a good many suppressed premisses, such as *implicit* generalisations and *tacit* initial conditions, premisses which are likely to be articulated, if at all, only when the particular explanation in question is challenged.

These remarks apply equally to the examples used by C. G. Hempel, one of the most frequently quoted proponents of the schema. ' Quite commonly ', Hempel notes:

> . . . the explanation offered for the occurrence of an event is incomplete. Thus, we may hear the explanation that a barn burnt down ' because ' a burning cigarette was dropped in the hay, or that a certain political movement has spectacular success ' because ' it takes advantage of widespread racial prejudices. . . . In explanatory statements like these, th general laws which confer upon the stated conditions the characte ' causes ' or ' determining factors ' are completely omitted . furthermore, the enumeration of the determining conditio incomplete . . . much more detailed statement of deter tions and universal hypotheses would require amplifi serve as a sufficient basis for the deduction of the c

Hempel adds that the generalisations are usua often concern familiar facts of individual a Popper elaborates:

[1] Popper, op. cit. pp. 59–60
[2] See, for example, op. cit. sectio
[3] Hempel, loc. cit. pp. 462 sq.

> . . . these laws may be so trivial, so much part of our common knowledge, that we need not mention them and rarely notice them. If we say that the cause of the death of Giordano Bruno was burning at the stake, we do not need to mention the universal law that all living things die when exposed to intense heat. But such a law was tacitly assumed in our causal explanation.[1]

Popper warns that these remarks about the application of the schema are not restricted to its use in *historical* explanation. We find a situation similar to treatment of individual events in history whenever we encounter an actual application of science to a singular event or specific problem. As Popper explains:

> The practical chemist . . . who wishes to analyse a certain given compound —a piece of rock, say—hardly considers any universal laws. Instead, he applies, possibly without much thought, certain routine techniques which, from the logical point of view, are tests of such *singular* hypotheses as ' this compound contains sulphur '.[2]

2

If the ' picture interpretation ' of the schema is the main source of misunderstanding about it, another, somewhat more interesting, logical error has been almost as harmful. This is the notion that proponents of the schema claim that a universal generalisation or law involved in explanation is of such a nature that (when expressed in conditional form) its antecedent consists of the conjunction of the initial conditions, while its consequent is the explicandum.

What is in fact claimed is that the *laws* involved in the model are themselves *members* of a set of statements in conjunction whose other members include the initial conditions. A conjunction of the laws and initial conditions would logically entail the explicandum. This may become clearer from the following analysis:

[redacted] *correct* interpretation of the schema is, schematically, the

(1) $\ulcorner L. \equiv . C_1, \ldots, C_n / E \urcorner$

[redacted] Poverty of Historicism', op. cit., p. 145

[redacted] ' C ' for ' conditions '; ' E ' for ' explicandum ' or [redacted] used to indicate ' logically entails '. (1), (1*a*), and (2) [redacted] R ', rule of inference, such that (3) $\ulcorner R \equiv .C_1, \ldots,$ [redacted] that the schema's proponents have (1) or (1*a*) in [redacted] opinion which recurs from time to time that, far [redacted] in historical explanations are usually ' too

An alternative, but *still erroneous*, formulation (which however takes account of the claim that the relation between antecedent and consequent of a law is stronger than material implication) would be:

$$(1a)\ \ulcorner L. \equiv .\ C_1, \ldots, C_n \rightarrow E\urcorner$$

On this formulation the law is equivalent to the statement that the effect to be explained follows *with physical necessity* from the conjunction of the initial conditions. I have distinguished between (1) and (1*a*) in order to acknowledge the present controversy about counter-factual conditionals and the nature of implication in natural laws. That distinction is, however, irrelevant to my argument, for the correct symbolic representation of the schema is this:

$$(2)\ \ulcorner C_1, \ldots, C_n, L_1, \ldots, L_n / E\urcorner$$

According to this formulation, the event to be explained is logically derivable from the conjunction of all the conditions and all the laws. Formulation (2) would resemble formulations (1) and (1*a*) only in those explanatory situations, if any, where *only one law is involved.* Even here there would be a difference, and in most explanatory situations, where *many* laws are involved, there is no resemblance.

For example, suppose that in order to explain the lighting of a match, it is pointed out that the match was struck. Some critics of the schema—those, for example, who would accept either (1) or (1*a*) as a correct formulation of it—have assumed that its proponents would have to claim that the tacit law, ' All matches light when struck ', had been used.

This assumption is seriously incorrect. In the first place, ' All matches light when struck ' is no law at all: it is quite false, however often it may be cited in philosophical literature as an example of a la The sceptic should experiment on a box of matches.

Rather, the model's proponents would claim that the fo was tacitly assumed in this explanation: given a number of of nature concerning friction, heat, etc., and given conditions about such things as the presence of oxyg

complicated and difficult to state '. This confusion, sometim refer to the difficulties of ' ceteris paribus ', underlies chap is perhaps his worst mistake. ' Ceteris paribus ' may specified members of the explicans, but never to the an law statement. This error seems even to occur in an model. See Morton White's article, ' Toward an *Philosophic Thought in France and the United Stat* p. 720, n. 22.

the degree of pressure exerted on the match, then the match would have to light. Furthermore, they would claim that the *failure* of the match to light would imply that at least one of the things given or assumed—whether initial condition or law—did not in fact obtain. On the other hand, proponents of the schema would not claim that they are using a *law* to the effect: 'Whenever a match is struck, and there is oxygen in the air, and the match box is not wet, and . . ., and the match head is not wet, then the match lights'.

I have stressed this point because confusion about it has been widespread and highly infectious, involving issues other than explanation. P. H. Nowell Smith, for example, once based an argument about the uniqueness of historical events on this mistake.[1] His reasoning goes something like this. Assume (1) that the explanation of each historical event is unique, i.e. that each historical event would be explained by a unique set of initial conditions which cannot occur again. Assume (2) that the schema's proponents claim that underlying each such explanation is a natural law whose antecedent (when the law is expressed in conditional form) is the conjunction of these initial conditions, and whose consequent is the event to be explained. It follows that this 'law' is *unique*. Thus, since laws are *universal* propositions, it is no law at all. Thus historical explanation does not involve laws.

Although certain objections might be raised about the conception of universal law which underlies Nowell Smith's argument, his reasoning itself seems to me to be unobjectionable. But at least the second step of his argument is false: it is based on the incorrect (1) or (1*a*) interpretations of the schema. I do not wish to criticise Nowell Smith on this matter too severely, for he seems to have rested his [illegible] on Gardiner's inaccurate (1*a*) rendering of the schema, to which [illegible]rs. On the other hand, Nowell Smith unfortunately did not [illegible] an exactly parallel argument would have brought him to [illegible]ling conclusion that the explanation of any particular [illegible]h as a temperature change—is unique and hence [illegible].

3

[illegible] clarificatory remarks will I think provide [illegible] objections raised about the hypothetico- [illegible] by no means sufficient in themselves to

[illegible]ical Events Unique?', *Proc. Arist. Soc.*, 1956–57,

[illegible]I

defeat the hundreds of separate arguments against it that have been lodged during the past decade. Fortunately, however, a fairly small cluster of typical, trivial, yet important mistakes seem to underlie most of these further objections. These objections seem to be made along two main fronts—conditioning events and laws—which I shall discuss in turn.

(1) The schema's proponents are wrong, it is said, to insist that historical explanation is made by referring to conditioning events. For much historical explanation occurs by referring to human motives and dispositions, which Gilbert Ryle—in his attack on Cartesianism—has shown not to be 'events'.[1]

(2) The schema's adherents are wrong in claiming that historical explanations require general laws. For it can be shown, by way of Ryle's discussion of the law-like character of dispositional statements, that statements concerning historical occurrences may be logically derived from dispositional statements *per se*, without the help of general laws.

To deal with these contentions in turn: the schema's proponents do not claim that only specific *episodic* happenings or events—in a sense of 'event' which excludes dispositions—qualify as conditioning statements. Hempel's choice of the word 'event' in his presentation of the schema in order to describe the initial conditions, was unfortunate. Yet it is clear from his own examples of an explanation that dispositional statements qualify as 'events' under his understanding of this word. This can be seen by examining the list of initial conditions he gives in his well-known radiator example: 'Its radiator, which *consists of iron*, was completely *filled with water* . . . the *pressure was normal*. The *bursting pressure* of the radiator mate so and so much.' Hempel classifies these initial conditions, differentiation of type, side by side with more episodic eve temperature during the night dropped from 39° F. left in the street all night.'

The initial conditions I have indicated by i events in Ryle's sense; they are, rather, parts of describing certain potentialities of the materia

It is understandable that someone mig after a superficial reading of his article, fo is somewhat misleading. But there i

[1] Gilbert Ryle, *The Concep*

confusion in the account by Popper quoted above. Instead of talking of 'events', Popper speaks of initial conditions as singular statements which 'apply to' or 'pertain to' the specific event in question (*The Open Society and its Enemies*, Vol. 2 p. 262). Dispositional statements clearly fit this description. Could one deny, for instance, that 'Smith was ambitious' is a singular statement 'pertaining to' the special case in question i.e. Smith's campaigning for political office; or that the fact that 'Smith was ambitious' may be an initial condition for understanding and explaining his campaigning?

A specific historical illustration of this point is provided by Sir Winston Churchill's explanation of the attitude of certain French circles in North Africa toward General Charles De Gaulle in 1942:

> In a state like France, which has experienced so many convulsions—Monarchy, Convention, Directory, Consulate, Empire, Monarchy, (Republic), Empire, and finally Republic—there has grown up a principle founded on the *droit administratif* which undoubtedly governs the action of many French officers and officials in times of revolution and change. It is a highly legalistic habit of mind, and it arises from a subconscious sense of national self-preservation against the dangers of sheer anarchy. . . . Much turns in the minds of French officers upon whether there is a direct, unbroken chain of lawful command, and this is held by many Frenchmen to be more important than moral, national, or international considerations. From this point of view many Frenchmen who admire General De Gaulle and envy him in his rôle, nevertheless regard him as a man who has rebelled against the authority of the French State. . . .[1]

Commenting on Sir Winston's explanation, Pieter Geyl remarks: 'This reluctance to admit the right of the individual to act in a crisis ...nst his government, which I have shown to be typically French, is ... means confined to the tradition of that country'.[2] Clearly ...itional attitude of Frenchmen, this 'highly legalistic habit ... Sir Winston puts it, 'pertains to' the problem in hand, ... *conditions* referred to in the explanation, even though ... 'event'.

...d, the fact that dispositional statements can, as ...ema, act as conditioning statements in explana- ...mean that Ryle's analysis of motives and dis- ...loned. For the schema itself is neutral

[1] ...*ves with Historians*, London, 1955, p. 208 sq.

concerning the analysis one might give to dispositional statements. Nor need we question the possible usefulness of Ryle's distinction between two kinds of explanation: one where an episodic event is emphasised in the explicans (such as the striking of one billiard ball by another, or a stone breaking a piece of glass) and another where explanatory emphasis is put on a dispositional statement (about, for instance, the mass of the billiard ball or the brittleness of the glass). Perhaps we might even distinguish a third kind of explanation, where both episodic event and disposition are involved importantly in the same explicans. The possible usefulness of such a sub-classification of explanations gives no support, however, to the idea that such a distinction provides an argument against the hypothetico-deductive schema. For the disagreement is purely verbal: where Ryle distinguishes two different types of explanation, Popper might say that there are at least two different types of initial condition involved in explanations.

So much for the schema's 'events'. The second line of criticism of the schema has to do with the 'laws' it involves, although dispositional statements also play an important rôle in this attack. The underlying mistake here has to do with a confusion over the ability of an explaining dispositional statement logically to entail its explicandum without the help of a universal generalisation or law. This confusion, which is a common one, can be explained in terms of the paper by Donagan cited above.

Donagan makes the trivial logical point that certain kinds of statement about one individual happening can be *validly derived* from statements about other individual happenings without the mediation of general laws. He writes:

> That theory (Hempel's) in fact rests on its foundations . . . only if it is true that a statement about one individual happening cannot be logically derived from statements about other individual happenings except by the mediation of general laws.[1]

Had anyone claimed 'that a statement about [illegible] happening cannot be logically derived from statements about individual happenings except by the mediation of [illegible]' one could agree with Donagan that the view is wr[illegible] trivial examples of valid inferences:

(I) Premiss consisting of a statement of individual happening: [illegible]

[1] Donagan, op. c[illegible]

	Conclusion consisting of a statement of an individual happening:	Hegel played Hop-Scotch with the Absolute.
(II)	Premiss consisting of a statement of an individual happening:	The cow jumped over the moon.
		If the cow jumped over the moon, Mann wrote *Joseph*.
	Conclusion consisting of a statement of an individual happening.	Mann wrote *Joseph*.
(III)	Premiss consisting of a statement of an individual happening:	*a* is an apple in the green basket on Ryle's desk at time *t*.
		Every apple in the green basket on Ryle's desk is red at time *t*.
	Conclusion consisting of a statement of an individual happening:	*a* is red at time *t*.

Example (I) speaks for itself. Again, neither 'If the cow jumped over the moon, Mann wrote *Joseph*', in example (II), nor 'Every apple in the green basket on Ryle's desk is red at time *t*', in example (III), is a general law. Yet, they enable us to derive conclusions about individual happenings from statements about other individual happenings. If the schema rested on 'foundations' that denied such elementary logic, Donagan and those other critics of the schema who have criticized his argument as valid need hardly have resorted to an analysis of conditional statements to make their point!

However, no advocate of the schema has ever denied the fact that a statement about one individual happening can be derived from statements about other individual happenings only with the aid of general laws. One can indeed validly derive from another statement which is logically weaker than itself. An objection made on these terms, far from refuting, indicates that perhaps the most important point has been missed. For instance, Popper believes that explanatory premisses should be as strong

as possible in order to avoid circular and *ad hoc* explanations. (Their logical strength, of course, is a very different matter from their familiarity, and even triviality: trivial and extremely familiar laws can often be very strong in logical and empirical content.) The reason for this is that the stronger hypothesis will be less *ad hoc* and more testable: able to be tested independently in situations other than the particular explanatory situation under consideration.[1]

From the truth that certain statements, such as dispositional statements, *logically entail* certain other statements without utilising general laws, it is a *non sequitur* to derive the conclusion that certain statements *explain* certain other statements without the use of a law. It is one thing to maintain that all explanation is deductive; quite another thing to maintain that all deduction is explanatory. That the critics of the schema thought they must resort to a very involved and perhaps not very fruitful investigation of dispositional statements in order to make an elementary point of logic may indicate how confused discussion of this subject has been.

In Donagan's particular case, the roots of the mistake lie even deeper—in his understanding of the notion of ' general law '. He writes:

> *A general law is a hypothetical proposition which is general in that it mentions no individual.* . . . If we use the word ' open ' as a technical term meaning ' containing no mention of individual things, happenings and so on ' . . . then laws are completely open. . . . (my italics.)

This is a different theory of ' general law ' from that of the schema's proponents. Since Donagan's remarks about general laws are made polemically against Hempel, I shall have to refer here to Hempel's theory of general law in order to rectify the distortion. Hempel written at length on this subject in a paper, ' The Logic of Explan written in co-operation with Paul Oppenheim.[2]

The following two quotations from this article illustrat Hempel's conception of ' law '—whether correct or right—differs from Donagan's rather unusual defi law as a ' hypothetical proposition which is gener no individual '.

[1] See K. R. Popper, ' The Aim of Science ', op. ci

[2] C. G. Hempel and Paul Oppenheim, ' The L *of Science*, 1948, **15**, reprinted in *Readings in the Ph* and May Brodbeck, New York, 1953

> 1. We shall construe the class of lawlike sentences as including analytic general statements, such as ' A rose is a rose ', as well as the lawlike sentences of empirical science, which have empirical content (p. 338).

(Hempel understands the word ' lawlike ' here to include all the characteristics of a general law with the possible exception of truth. This usage, of course, differs from both Donagan's and Ryle's.)

> 2. A true general statement about the free fall of physical bodies on the moon, *while referring to a particular object, would still constitute a law*, albeit a derivative one (p. 340; italics mine).

In fairness, I should mention that I do not believe that Hempel's characterisation of general law is adequate. The point, however, is that whatever one's conclusions about Hempel's views, Donagan's argument that a very different kind of ' law ' is not required to derive one singular statement from another is superfluous. For Donagan contends that there are certain statements (namely, dispositional statements) which *logically entail* certain other statements without utilising general laws—' general law ' being understood in a sense which is claimed to be Ryle's. From this it is claimed to follow that certain statements *explain* certain other statements without the use of a law—' law ' being understood in Hempel's different sense. This is a double *non sequitur*.

Moreover, Donagan's characterisation of general law is intrinsically unacceptable. For neither Kepler's laws of planetary motion nor Galileo's law of falling bodies, to give only two examples of laws mentioning individuals, seem to be ' laws ' in his sense of the word.

4

hough some intrinsic interest attaches to the arguments reviewed ised in the last three sections, particularly in the context of con- hilosophical debate in Britain, it must seem ' curiouser that so many of these misunderstandings have been so ugh a general explanation of the discussion is this paper, I believe that it can be shown that one f this confusion can be found in the far from ssor Ryle.

tters, as in the discussion of ' events ', I n in context, did not necessarily conflict een misapplied by some of its critics.

Quite the contrary is true, of some influential remarks he has made about the rôle of hypotheses in arguments, to which I shall now turn. His remarks are pertinent here not only because of their implications for the problem of *historical* explanation; if his views were correct the hypothetico-deductive schema would be totally inadequate for *science* as well as for history.

In his article 'If, So and Because',[1] Ryle denied outright one of the most important features of the schema: the inclusion of universal hypotheses or natural laws in the premisses of explanatory arguments. Moreover, he warned that the attempt to use them as premisses was not only mistaken, but led to the logical absurdity of an infinite regress.

If universal hypotheses do not act as premisses in our explanatory arguments, what rôle then do they play? Ryle states that they are 'inference licenses' in accordance with which one argues from, say, p to q. Instead of deducing q from the conjunction of p and some laws, we deduce q from p 'in accordance with' laws. Ryle maintains that in this respect natural laws are analogous to logical rules: these too—the principle of the syllogism for instance—cannot be incorporated as premisses into arguments without leading to an infinite regress.

Although Ryle's view has been widely accepted, I know of no attempt to explain what it involves and how it is to be understood, nor of any effort to provide the view with some argumentative support. Rather, Lewis Carroll's puzzling essay, 'What the Tortoise Said to Achilles',[2] is regularly appealed to as if it were an authoritative and lucid demonstration of the view's correctness.

Carroll's story itself is well-known. At the end of their famous race, Achilles and the Tortoise pause for a philosophical chat. A once, the indefatigable Tortoise dictates the following three s ments to the dutiful Achilles, who writes them down in a not

(A) Things that are equal to the same are equal to each

(B) The two sides of this Triangle are things that ar same.

(Z) The two sides of this Triangle are equal to

The Tortoise points out to Achilles that a p accept the conclusion (Z) on two different gr

[1] Printed in *Philosophical Analysis*, ed. Max Black

[2] *Mind*, 1895, **4**; reprinted in *World of Ma* York, 1956. Vol. IV, pp. 2402-2405

the *truth of the premisses;* or, accepting the premisses as true, he might deny the *validity of the inference* from premisses to conclusion. The Tortoise then asks Achilles to imagine that he (the Tortoise) is a person of the second kind, and challenges Achilles to force him logically to accept Z as true. In other words, the Tortoise is to be imagined as a person who accepts A and B as true but who denies the ' hypothetical ' proposition:

(C) If A and B are true, Z must be true.

Through his spokesmen, Carroll indicates that even if the Tortoise can be induced to accept C as true and to incorporate it as a premiss into a new argument in which A, B, and C are premisses and Z the conclusion, the Doubting Tortoise still could refuse to accept Z. For he might not accept a further hypothetical:

(D) If A and B and C are true, Z must be true.

And so on, *ad infinitum.*

Without any hesitation, Ryle extracted the following lesson from Carroll's story:

> . . . as the Tortoise proved to Achilles . . . (the) principle of an inference cannot be one of its premisses or part of its premiss. Conclusions are drawn from premisses in accordance with principles, not from premisses that embody those principles. . . . It is not merely that the officially recognized Rules of Inference cannot be given the rôle of premiss components in all the specific inferences that are made in accordance with them. The same thing is true of the most ' meaty ' and determinate hypothetical statements . . . in saying ' *q*, because *p*,' we are not just asserting but *using* what is expressed by ' if *p*, then *q* '; we are putting it to work or applying it . . . just as the inference ' *p*, o *q* ' does not embody ' if *p*, then *q* ' as a component of its premiss, rather applies it in being an operation with ' *p* ' and ' *q* ' executed formity with it, so ' *q*, because *p* ' does not embody ' if *p*, then *q* ' onent of its ' because ' clause but applies it in another way.[1]

ulmin has endorsed Ryle's ' inference license ' theory e as follows:

ed in his paper, *What the Tortoise said to Achilles,* usions one is led into if one treats the Principle of r-major premise, instead of as an inference- with laws of nature. The conclusions about

[1] cause ', op. cit. pp. 328, 331

> the world which scientists derive from laws of nature are not deduced from those laws, but rather drawn in accordance with them or inferred as applications of them. . . . Certainly they act hardly more as premises in physical arguments than the Principle of the Syllogism does in syllogistic ones.[1]

I disagree with the interpretation of the story given by Ryle and Toulmin and with the belief that the story is relevant to the problems of explanation. As for the story itself, I do not share the view that there is one clear interpretation of it and its intended moral.[2] It seems most plausible to understand it as an attempt on Carroll's part to express some difficulties he felt but could not adequately explain; his failure in turn being largely due to the inadequacy of the logical theory available to him—lacking as it did the modern distinction due to Tarski between object-language and metalanguage.

On the basis of modern discussions of the ways in which the validity of an inference may be questioned and defended we might illustrate the following possible lesson with the Achilles story (although, I am afraid, the argument situation described in Carroll's story is so highly artificial that the illustration would not be a particularly good one): *Given a challenge to the validity of an argument, we give no defence by simply adding in, as another premiss, a version of the logical rule in accordance with which the argument proceeds.* The validity of an argument, as opposed to the truth of the conclusion, must be defended

[1] S. E. Toulmin, *Introduction to the Philosophy of Science*, London, 1953, p. 102. A similar argument which also refers to Lewis Carroll appears in R. Harré, *An Introduction to the Logic of the Sciences*, London, 1960, pp. 80 sqq. Toulmin's rejection of the hypothetico-deductive theory of science is especially strange. For example, he reasons from the correct opinion that laws cannot be deduced from observation statements, or translated into a set of them in a phenomenalistic way, to the incorr opinion that observation statements cannot be deduced from law premisses. C pare pp. 40-41 and 84-85. This is a *non sequitur*.

[2] Discussion and interpretation of Carroll's puzzle have been rather cu example, in her book *Lewis Carroll*, London, 1947, p. 270, F. B. Lenn identify it with two other problems: the Zeno problem of infinit problem of presuppositions, or of the defence of our 'ultimat criteria, which was implicit in Aristotle and perhaps first Sceptics, especially by Carneades and Agrippa (see his tropes). For the latter, consult Sextus Empiricus: *Out* G. Bury, London, 1933, Book I. These three proble common feature—i.e. a potential infinite regress c over, the first is indeed connected indirectly wit problems. For an attempted solution to the pr IV and V of my book *The Retreat to Commit*

metalinguistically. In order to defend a derivation, one would have to show metalinguistically that no counter-example exists for any interpretation of the inference which is form-preserving and whose premisses are true.[1]

Such a defence may, of course, be anything but a straightforward affair. Indeed, a number of extremely important further problems and difficulties arise in connection with the evaluation and defence of the validity of arguments. Controversy may begin over the admission of proposed counter-examples: for example, is the Cheshire Cat a counter-example to the King's Rule that 'anything that had a head could be beheaded'?[2] Argument may continue over which signs are to be interpreted as descriptive, and which as formative. And that demarcation may in turn become controversial—as Carroll himself well knew.[3]

Moreover, if such argument is conducted in the context of what I have elsewhere called a 'justification philosophy',[4] which makes the rationality of an argument dependent on its positive justifiability, a vicious infinite regress may easily be generated in the metalanguage. It can indeed be shown through the device of the infinite regress that certain views about the possibilities of demonstrative argument and 'being forced to a conclusion' are incorrect. But as I have tried to show in the place cited, such a justificational concept of argument can be avoided; and in any case an infinite regress so generated would be quite different in character from that illustrated in Carroll's story, in which the distinction between object-language and metalanguage is not made.

Yet even if Carroll's story *were* a perfect illustration of some important logical dilemma it is difficult to see how such problems [illegible] *valid inference* and *proof* bear in any direct way upon the concept [illegible]*factory explanation*—or how they support at all the comments [illegible]yle and Toulmin about the rôle of universal hypotheses [illegible] *For the explanation schema is not a picture of a standard* [illegible]*tration.* . When one explains *q* by citing *p*, one is not [illegible]k of finally proving that *q* happened or forcing a

[1] On the Concept of Logical Consequence', and 'The [illegible]ised Languages', in *Logic, Semantics, Metamathematics*, [illegible]r, 'Logic Without Assumptions', op. cit. especially

[2] *Alice in Wonderland*, chap. VIII

[3] [illegible]pula, and on the universal and existential quanti-

[4] See my book, *Retreat to Commitment*, loc. cit.

Doubting Tortoise to accept q. Rather, the assumption is that q happened; what is required is a highly testable set of premisses from which q may be *derived* (not *demonstrated*).

Once this has been understood, it is easy to see that the mere addition of some further premisses—including natural hypotheses of universal form—cannot in itself lead to any infinite regress. For the existing argument is either valid or invalid. Suppose it is valid. In accordance with the logical rule of Augmentation of Premisses, we can add *any* premiss we like to a valid argument without affecting that validity. The rule of Augmentation of Premisses may be expressed thus:

$$\ulcorner a_1, \ldots, a_n/b \;.\rightarrow.\; a_1, \ldots, a_{n+1}/b \urcorner$$

To give an illustration in ordinary English: If the conclusion ' Socrates is mortal ' can be derived from the two premisses ' Socrates is a man ', and ' All men are mortal ', then ' Socrates is mortal ' may also be derived from the augmented set of three premisses: ' Socrates is a man ', and ' All men are mortal ', and ' Socrates is a philosopher '. To take another example, ' Socrates is mortal ' may be validly derived from the set of premisses: ' Socrates is a man ', and ' All men are mortal ', and ' The moon is made of green cheese '. It is interesting to note in this connection that the first three statements dictated by the Tortoise, i.e. A, B, and Z, did constitute a valid argument from Euclidean geometry.

But what if the original argument is not valid? Could th addition of a natural law premiss to an *invalid* argument lead to logic paradox? Provided the added premiss is consistent, the answer these questions is also negative. At best, the addition of the l statements to the existing argument will render it valid; i. conjunction of the law premisses and the others will logica the conclusion. At worst, the altered argument will re

What leads to an infinite regress is not the use of statement, be it logical rule or natural law, to streng an argument, whether valid or invalid, but the thereby finally establish or prove the truth o validity of an argument.

One final explanatory comment and qu Behind the arguments of Ryle and Toul Achilles story is relevant here is a fa science. On such a view, scientific

not as true or false statements but rather as ' transformation rules ' for getting one empirical observation statement out of another. This is not the place to discuss the advantages and disadvantages of instrumentalism. But even if we were to assume the instrumentalist view as correct—i.e. even if scientific laws were *no more than* rules of inference—it might be asked whether it follows that scientific laws, so interpreted, would be sufficiently analogous to *logical* rules of inference to support the contention that a logical difficulty that is supposed to apply to one will also apply to the other ?

5

In sum, I believe that the criticisms of the application of the hypothetico-deductive schema to historical explanation are invalid. Of course, it remains possible that there will in future be found some explanatory activity carried on in the study of history which the schema cannot handle. One such contender might be an ' explanation of what one means ', since almost any historian occasionally follows up a surprising statement by explaining how he was using his terms. This kind of ' explanation ', of course, also occurs in the sciences; and it can hardly be called *causal* explanation in either case.

If this newly discovered explanatory activity—whatever its character—proved important, it might be found that the importance of the hypothetico-deductive schema had been exaggerated. But the conclusion that this would prove that a basic difference exists between logical structure of historical explanation and that of explanation natural sciences would not follow unless, as pointed out at the g of this paper, it had also been shown that the same kind of n does *not* occur in sciences other than history.

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THE LOGIC OF COSMOLOGY*

MILTON K. MUNITZ

To anyone aware of the long history of cosmologic speculation and of the apparently interminable battles over the questions concerning the finitude or infinity of the universe in space and time, the situation faced in recent scientific cosmology contains great drama and significance. Are scientists, we may ask, really on the verge of discovery of the spatial and temporal structure of the universe that would lay to rest the endless disputations of the past? Can empirical inquiry really give us secure knowledge about the universe such as men have thirsted for ever since they lifted their eyes toward the heavens and sought by their wondering minds to find some answer to this profoundest of all riddles? The situation is clearly of the greatest intellectual import. However, precisely because cosmology promises so much, analysis of the *kind* of knowledge we are being offered is particularly necessary. And this is the point at which philosophy can properly enter the picture. A philosophical approach to cosmology should not presume to judge the relative merits of competing claims made by rival cosmologies, nor engage directly in the evaluation of the empirical evidence. Happily this may be left to the experts themselves. The relevant ta of philosophy with respect to cosmology instead concerns itself raising a number of special questions, such as the following: kind of means are employed by cosmology to give us an unde of the universe? What bearing does the observational evi upon the confirmation of cosmological hypotheses? mean to speak of determining the temporal and s the universe? Indeed, what does it mean to *has* a spatio-temporal structure? In particu evolutionary hypothesis should come to be observational evidence, and one says in ac that the universe has a finite age, how is s or, similarly, if some accepted cosmolog

* Read to the History and Philosophy of 7 March 1961 and to the British Society for

the universe is finite and bounded in space, how is this claim to be understood? In these cases, shall we have been provided with a form of knowledge that is logically in the same class as the kind of knowledge that the earth had a finite origin in the past, or that the earth is spherical?

In what follows, I shall not try to deal with all of the various issues of a philosophic character that are posed by the procedures and aims of scientific cosmology. I shall confine myself instead to some very general comments about the overall method or logic of cosmology, in terms of which any more detailed investigation of special problems and concepts would have to be undertaken. Even here my comments in the present discussion will be largely of a negative character, in the sense that I shall try to clear away some obstructions in the way of our understanding of what cosmology is. I believe, however, that such a preliminary negative analysis is essential, if one is to go on to a more constructive and positive account of what cosmology can accomplish.

I

Scientific cosmology is commonly defined as that branch of physics or theoretical astronomy that is engaged in the study of the properties of the universe, where the universe is thought of as being the most inclusive or complete physical whole. To say that the universe is the most complete physical whole means that whether we think of it as a lass of physical objects, as a physical system bound together by struc- l principles that interrelate its parts in some orderly scheme, or as hing having a spatial and temporal structure of its own, the will not form part of some more inclusive physical class, spatio-temporal structure. In this sense, by virtue of what speaking of the universe as the most complete physical ique system. To say that the universe is unique means t one such system and not more than one such

of the logic of cosmology we are confronted, ature of the subject matter to be examined, e universe. This should put us on our n identification of the logic of cosmology In other branches of physics, such as nics, electromagnetism, and so on, as

the discovery of *laws* and *theories*. Cosmology, however, as we shall now undertake to show, is not interested in the discovery of either laws or theories, and we shall therefore want to isolate and examine its own special intellectual device for understanding the universe.

The phenomena studied in ordinary branches of physics are of such a kind that a distinction can readily be made between the common features to be found among a variety of instances of some particular phenomenon, that is, what is expressed by the observationally established law of such a phenomenon, as contrasted with the differentiating individual characteristics that distinguish one instance or occurrence of the phenomenon from another. In mechanics, for example, we draw a distinction between the law of freely falling bodies, which states the invariant features to be found in all instances of such motion, and the individuating features that distinguish one case of a freely falling body from another. Such individuating circumstances are to be found in the values assigned to the initial velocity and position that identify one particular case of such motion as contrasted with another. To take another example: the study of astrophysics deals with the recurrent features in the form of laws that express interrelations among various items of selected interest, such as the luminosity of stars, their mass, spectroscopic patterns, evolutionary sequence, and the like. By contrast, the detailed study of the Sun or some other individual star is at best the application of the laws of astrophysics as brought to bear upon such a particular instance.

The principal uses to which laws of some science can be pu
when discovered and confirmed are explanation and prediction w
respect to the individual instances that can be brought within
scope. Given some already observed feature of a particular i
of some phenomenon, one can explain this observed fact b
that it can be deduced from the statement of the law (or la
to this instance together with the appropriate initial
the particular case under investigation. We can ex
why it did take so many seconds for this freely
the ground since this fact follows from the con
of the law of falling bodies and the statem
conditions, namely the particular height
dropped and its initial velocity. Simila
purpose of prediction, is to show how s
of a particular instance of some pheno
tory to be followed

can be deduced from the conjunction of the general law for projectiles and other already given or assumed particular features of the case under investigation, such as the projectile's initial velocity, angle of elevation, mass, and so on.

With respect to these familiar uses of laws in providing explanations and predictions, not only can we readily make a distinction between the general law and the actual instances to which the law applies, but we should also mark it as a prerequisite of the very meaning and use of such laws that we be able to refer to an actual or at least possible *plurality* of instances to which the law applies. For unless there were a plurality of instances there would be neither interest nor sense in speaking of a law at all. If we knew that there were only one actual or possible instance of some phenomenon it would hardly make sense to speak of finding a law for this unique occurrence *qua* unique. This last situation however is precisely the one we encounter in cosmology. For the fact that there is at least one but not more than one universe to be investigated makes the search for *laws* in cosmology inappropriate.

In so far as the universe, by definition, is regarded as a unique object or system, it cannot be treated as an instance of some recurrent phenomenon. Cosmology does not undertake to establish laws about *universes*. Hence the distinction that is appropriate in other contexts between the individually differentiating features of multiple instances of some phenomenon and what they all have in common, does not apply. For the universe as a unique system, the distinction between what is individual and what is universal breaks down. At best one can establish laws about the *constituents* of the universe rather than laws about the universe. Whereas other sciences, however, are interested in the various constituents of the universe, cosmology is interested in the universe itself as the unique configuration of its major astronomical parts.

This last point helps us to note and correct a certain looseness in the use of the expression ' laws of the universe '. Thus, if we should want to say that a law such as Newton's inverse square law of gravitation is a law of the universe, what we should properly mean, of course, is that at best it holds *universally*. This is to say, for any two bodies *in* the universe, there will be a relation holding between them such that they attract each other inversely as the square of their distance and directly as the product of their masses. In the same way, a law of thermodynamics, such as the Second Law, is not, strictly speaking, a

law of the universe. The various inferences [illegible] for example, about the 'heat-death' of the universe or its 'r[illegible] down' on the basis of this law, are just so many myths. They [illegible] from confusing the status of a law which is claimed to hold universally, and something which can be claimed to hold for the universe itself. The fact that the Second Law of Thermodynamics holds for any closed system found within the universe and so to systems which have an environment, does not immediately sanction the application of this same law to the universe itself which by definition has no environment. While then the phrase 'law of the universe', as in the above examples, is simply meant to emphasise the unrestricted scope of the instances to which the law applies, its universal range, such laws are not strictly laws *about* the universe; they are about various entities, processes, or events *within* the universe.

One might ask, however, at this point, whether there is any connection between laws of the universe in the sense of laws holding for the constituents of the universe and the universe itself. Now various schemes of recent cosmology have indeed attempted to establish some logical connection between laws of the universe, in the sense of statements of unrestricted scope having to do with entities in the universe, and the universe itself considered as the unique and most inclusive system of all material bodies. Some cosmologists, for example, have argued that it should be possible to show that the ordinary laws of physics can be linked with the properties of the universe as a whole. The universe would disclose itself to be a unitary physical system o[illegible] such a kind that it would be possible to deduce ordinary physical la[illegible] from the principles of cosmology. Milne, for example, in his [illegible] matic model of the universe, undertook to show how the [illegible] square law of gravitation could be deduced from what he re[illegible] fundamental cosmological principles holding for the u[illegible] whole. Similarly, Sciama, by employing a steady [illegible] suggested how one should be able to link the in[illegible] matter in any local region with the distribution [illegible] verse at large. But even were some degree o[illegible] efforts, the point we have been making ab[illegible] laws which hold for entities *in* the univ[illegible] purport to say something *about* the univers[illegible] physical system, would still hold. Fo[illegible] complished in such a linkage of the or[illegible] structure of the universe as a whole,

be described in terms of a law or set of laws. What it would mean is that whatever special devices we use for comprehending the structure of the universe as a whole can be employed not only to give us a picture of the structure of the universe as a whole, but can also be made to serve as an explanatory means for laws of the *constituents* of the universe. We should then look for the explanation of laws not simply, as we ordinarily do, to the *theories* of physics, but to the distinctive power of cosmological models to give us such explanation.

Now before leaving this general point about the usage of the phrase 'laws of the universe', I think it worth noting that the phrase is also sometimes used deliberately to refer to something holding for the universe itself and not simply for constituents of the universe. According to some, for example, the curvature of the universe as a whole, which may be negative, zero, or positive, is specified by a law of the universe. It would seem to me, however, that such a usage is ill-advised. For the notion of law implies repeatability, invariance of pattern in multiple instances, whereas something like the curvature of the universe is not a repeatable pattern in multiple instances. It is at best a single feature of a unique 'object'. Since the universe is unique, it makes no sense to speak of laws of, or about, the universe. With respect to the universe one cannot obtain the kind of empirical evidence that is possible in connection with laws, namely that which is obtained by an examination of common traits to be found in a plurality of instances. In particular, the relation in which the observable portion of the universe stands to the universe is as *part* to presumed *whole*, rather than of *instance* to *law*.

The objection might be raised at this point that to deny an interest part of cosmology in the formulation of laws is unwarranted, matter of fact one does find distinctively cosmological laws. haps the best known instance is Hubble's law, which states confirmed uniformity that the apparent magnitude of correlated with the red-shift in their spectra. Now d to deny that one may legitimately designate Hubble established by empirical means as a r, strictly speaking, a law in cosmology or , rather, a law whose domain of reference s individual units. As such, it is a law of of cosmology. What it asserts is that agnitude is measurable and for which, able for the red-shift in its spectrum,

quantity $\dot{R}_0/R_0$ where the latter is a purely conceptual construct whose meaning is guided by the use of a geometric analogy, involving the idea of 'expanding space' and is therefore not defined operationally at all. Whereas the statement of Hubble's law can be treated as an ordinary empirical generalisation, its use for the purpose of cosmology changes it into something which at best gives a numerical value to a geometrically inspired construct, where the latter is itself part of a network of similar constructs. We may conclude then, that for the reasons given, cosmology cannot be said to treat of anything which can properly be called empirically established *laws* of the universe.

Another important aspect of the methodology of ordinary physical sciences, in the more advanced stages of their development, is the formulation of well-tested, comprehensive *theories*. Theories make possible the explanation of laws already known, and the 'prediction', that is the discovery of hitherto unrecognised empirical regularities. Examples of theories in this sense are the Newtonian or Einsteinian theories of mechanics, Maxwell's theory of electromagnetism, the kinetic theory of gases, and quantum theory. As distinguished from the concepts that are made use of in the statement of experimentally established laws, the concepts that figure in these theories are not initially defined by reference to some laboratory procedure or to some observationally identified trait of some subject matter. What is meant, for example, by such theoretic concepts as 'force', 'space-time curvature', 'electromagnetic field', or 'electron' cannot be determined by some direct experimental procedure or observational appeal, in the way in which, by contrast, one gives meaning to such empirical concepts as 'length', 'velocity', or 'temperature' in the statement of experimental laws. When a theory is expressed in the form of an axiomatised deductive system, the meaning of the logically primitive concepts in such a system is provided by the axioms or postulates of the theory in which these concepts appear. Such postulates in effect constitute implicit definitions of these concepts very much in the way in which, in a rigorously formal statement of Euclidean geometry, for example, the postulates of that geometry constitute implicit definitions of the otherwise primitive and undefined terms such as 'point', 'line' and 'plane'. Thus terms such as 'force' and 'mass' as conceptual constructs in Newtonian mechanics are implicitly defined by the postulates of that theory, namely the three Laws of Motion. As in any formalised scheme, moreover, while some terms will be primitive and defined only implicity by the axioms of the system, other terms

will be explicitly defined by means of the primitive or undefined terms, and so will be ultimately replaceable by the latter. In order for a theory to function as a useful account of some empirical subject matter, it is necessary to establish lines of connection between, on the one hand, at least some of the theoretic concepts introduced by the theory (whether these be primitive terms in the system, or terms explicitly defined intra-systemically by reference to the primitive terms) and, on the other hand, empirical concepts as used in the statement of empirical laws and that are already defined independently of the theoretic system. The latter empirical concepts will have been defined, that is to say, by reference to some overt operational procedure, process of measurement, or perceptual means of identification. Such lines of connection that correlate specific theoretic concepts with specific empirical concepts are expressed by means of a type of 'dictionary'. The 'dictionary' is made up in effect of various rules of correspondence, semantic rules, or co-ordinative definitions. Each such definition or rule says that to such and such a theoretic concept, for example, 'kinetic energy of molecules' there shall be made to correspond such and such an empirical concept, for example, 'the temperature' of a gas (as measured by a laboratory thermometer). Having established bridges between the theoretical scheme and the empirical concepts that appear in the previously confirmed empirical laws, one may not only provide another conceptual terminology for expressing the relations of the law itself, but more importantly, the law so re-expressed or translated into the conceptual terminology of the theory may then be shown to follow from the initial postulates of the theo By having made the appropriate substitutions of conceptual formula the deduced consequences of the initial postulates of the theory found to contain statements which correspond to the a dependently established empirical laws. But to show that deduced from the theory is to show that it can there Thus the observationally established laws of the leve of the planetary orbits, of freely falling bodies, o of others, were explained by first being transl terminology of Newtonian theory and the mental postulates. In a similar way, the laws of electrostatics or of simple magne were explained by Maxwellian theory logical laws of gases were incorporate kinetic theory of gases.

Now the point of recalling the foregoing well-known features of theories is this: since theories are used primarily to systematically explain laws, and laws in turn have a significance only because the phenomena they deal with are exhibited in plural instances, the logic which characteristically belongs to sciences that depend on the fact of the repeatability of some phenomenon cannot be extended without important qualifications to the field of cosmology. Not only is cosmology not concerned with the discovery of laws for the purpose of 'explaining' the universe, it cannot even be said to be interested in the discovery of a *theory* of the universe, in the sense in which we have been using the term 'theory', that is to say, as a name for the conceptual means primarily employed for the explanation of laws. The aim of cosmology rather is to give some reasonably coherent and complete account of what the actual unique universe is like. The cosmologist, just as any other empirical scientist, starts from an empirically identified domain of observed facts for which he seeks intelligibility. But in his case the facts he starts with are not individual occurrences (instances of some phenomenon) nor even some observed regularity or collection of regularities. The cosmologist begins with data obtainable about the actually observable region of the universe whose limits are set by the power of the astronomer's instruments. The cosmologist does not wish to give an explanation for this observable region of the universe. He does not treat this observable region as if it were an instance of some phenomenon which he is trying to understand or explain in the sense of showing that it follows from or in accordance with some law. Nor does the cosmologist treat the observable region as some kind of law or collection of laws which he would try to explain by showing that they follow from or in accordance with some theory. The observable region of the universe is neither an instance of a law not is it a law or a collection of laws. It is simply a domain of observed entities. What the cosmologist finds insufficient about his grasp of the observable region is precisely that the observable region is incomplete. What he looks for is some way of understanding it as completed, in the sense of seeing how the observable region forms part of a whole whose complex pattern he can specify. The intelligibility cosmology looks for accordingly is of the kind which involves grasping the structure of a whole of which at present only a part is given. The primary intellectual tool at the disposal of the cosmologist for achieving this goal is the use of a *model of the universe*. A model of the universe serves a primarily different

function from both laws and theories. Its primary function is not to explain the universe so much as it is to describe it. The intelligibility offered is a 'descriptive intelligibility' rather than an 'explanatory intelligibility'. However, the term 'description' must here be understood in a very broad sense, as including the use not only of terms of observational astronomy, but highly abstract, conceptual constructs as well. It is within this primarily descriptive context of cosmology as provided by a model of the universe as a whole, that such explanations and predictions that cosmology in its own way is able to make, find their place.

It will be helpful at this point to make a terminological remark in connection with the use of the term 'model'. For there is a widespread usage of the term 'model' which connects it with an important component in the development of *theories* as we have briefly defined 'theories'. One speaks here characteristically of a 'model *for* a theory'. It is with such a usage of the term 'model' that I wish to contrast the way in which the term 'model' is normally used in cosmology, and as I am using it here. To speak of 'a model *of* the universe' is to speak of something which has a different role and status from being a model *for* a theory. A model for a theory is to be thought of as a subsidiary component of the theory which helps in the construction, interpretation, and extension of the theory itself. The model for the theory, however, is not the theory and must not be confused with the latter. It nevertheless plays an important role in inquiry. First of all, it guides the investigator in the construction of the theory. It helps interpret the basic ideas of the new theory by offering an analogy with a domain of familiar experience or with a previously developed theory which has already been employed in connection with a different subject matter from the subject matter under immediate investigation. In the second place, fruitful models for theories continue to serve as a storehouse of suggestions for the modification and extension of the original theory. A model for a theory, then, is an associated component of the theory without which the theory would neither have been constructed in the first place nor continued to grow by an enlargement and improvement in the details of its formulation. Bohr's planetary model of the atom represents a familiar example of a model for a theory in this sense. It exemplifies the way in which, at one stage in the development of atomic physics, the ideas of classical dynamics were suitably modified to deal with the facts of the discontinuity of radiation and the observed data having to

do with the line spectra of various elements. Other familiar examples are the use of hydrodynamics to suggest a theory of heat conduction, or of classical particle mechanics to suggest a theory of the molecular constitution of gases.

By contrast with models in this sense, I wish to stress that the term 'model of the universe' as used in cosmology, does *not* represent a subsidiary or associated element *in* a theory but is itself the name to be given to the principal device used for understanding of the universe. Indeed, we may even turn the matter around, and instead of speaking of a model *for* a theory, say that in cosmology we need to recognise the way in which established physical theories serve the needs of constructing and interpreting a model of the universe. We may thus speak of a theory *for* a model, in the sense that in obtaining ideas in terms of which to describe the structure of the universe as a whole, the cosmologist will typically turn to the well-established ideas of some physical theory to provide the guiding principles *for* his model *of* the universe. For in the construction of models of the universe, analogies play as crucial a role as they do in the construction and development of theories. Since the universe as a whole is not given as a datum or object of some sort that can be inspected, the only way in which cosmology can nevertheless undertake to describe it as a whole is by means of analogy. The cosmologist will in effect say that if we are to 'understand' the universe as a whole we must conceive it as similar in structure to something with which we are already familiar. Here we come upon an important point in the historical evolution of the bject of cosmology: whereas primitive cosmogonies and cosmologies le an appeal to all sorts of dramatic and anthropocentric analogies, n scientific cosmology turns instead for its analogies to the of mathematics and the physical laws and theories of other physics. Although there is an important transformation r of the analogies appealed to, it must not be overlooked even the most advanced and sophisticated of current rse is still analogy. For any attempt to construe se as a whole rests upon the mind's readiness to itable modifications what it has found useful ng its experience in certain special domains e nature of the whole. Thus when, as in e subject, cosmologists undertake to as- of the universe, or to decide whether emporal pattern' one should think of

the universe as having undergone an 'evolutionary development' from some unique 'origin' in the past of 'cosmic time' or as exhibiting an 'endlessly cyclic pattern of expansions and contractions', or as always remaining in a 'steady state', these and other such suggestions are at bottom expressions of fundamental analogical ways of construing the nature of the universe as a whole. They rest upon the use of concepts which need to be thought of as having the status of theoretic constructs, of precisely the same type as one encounters in the construction of theories. They are not empirical concepts defined by reference to observable traits of the subject matter such as make their appearance in the statement of empirical laws or singular judgments of fact. Any attempt, accordingly, to impute a literal significance to such terms as 'age of the universe' or 'spatial curvature of the universe as a whole', for example, would be a gross misapprehension of their logical status and role.

2

In the foregoing discussion I have argued for the value of distinguishing for logical purposes the kind of role that models of the universe play for cosmology from the role played both by laws and theories in the explanatory sciences. It is with the use of such models of the universe that the efforts of scientific cosmology to render the universe intelligible are bound up. Any philosophic approach to cosmology, therefore, does well to concentrate on this feature of the subject. For a philosophic approach to cosmology should undertake to make clear in what ways precisely the universe *is* made intelligi by the use of such models, and what the power and limits of sci in this area may be said to be. One particularly crucial way of c to grips with these questions is to focus on the problem of w not models of the universe are to be judged in terms of t *truth* or *falsity*.

As a possibly helpful device for approaching th may begin by noting the important differen involved in constructing a model of the univ constructing a model of something else. consider what is involved in making and u some object. Broadly speaking, there ar prompt the building of scale models. the original already exists, and, for ex the original will be built for purpos

instruction. The model is a physical analogue of the original, in which are duplicated at least some of the relevantly important features of the original. Such is the case where one makes a model of a well-known ship or building. In the second type of situation, models are constructed when the original does not yet exist. Thus a small scale model may be constructed in order to perform various tests upon it, as for example, when models of a projected airplane are made in order to perform wind-tunnel or other experiments. Here the model physically embodies some features of the 'original' where the latter as yet is only conceived or in the planning stage. In such cases, one has to transcribe from the blueprints of the drawing board onto the plane of physical existence something which will serve as a model for an object that is not already in existence. When the fully completed object is finally constructed, we should then normally refer to this as the 'original', and there would then be a relation between model and original that is identical with the first case mentioned earlier. Again, what makes the model a model is the fact that there are structural and functional similarities in the model and in the original. We look for those aspects of similarity that serve the particular purposes for which the model was constructed. Certain properties of the model may be wholly irrelevant to the purposes for which the model was constructed. For example, the model of a ship may be made of plastic, whereas the original was made or is to be made of steel or wood, and this may be a good enough model, if all we are interested in, for example, are [illegible]he general lines of the ship, its dimensions, and shape. With respect [illegible]this simple type of situation, then, the following points may be [illegible]: (1) The original can be identified independently of the model. [illegible] give a complete description of the features of the original [illegible]aving to resort to the model. Indeed, it is from the in-[illegible]he original that we obtain our knowledge of and clues [illegible] incorporated into the model. (2) We can inspect the [illegible]ed and give a complete description of *it*. (3) We [illegible]nal with the model in order to see how faithful [illegible] former.

[illegible]here are important differences between the [illegible]been examining and what is involved in [illegible]universe. Two such obvious differences [illegible]es.

[illegible]ind of model which the cosmologist [illegible] one rather than a material one. A

model of the universe is something which consists of *symbols* of one sort or another. These include ordinary language, mathematical language, diagrams, and charts. Indeed it is normally the case that all of these will be employed in constructing or expounding a given model. If, therefore we should wish to understand the model we should need to turn to the combined use of all of these devices. A model of the universe in this sense, therefore, will *not* be something that can be directly visualised or completely represented in a pictorial diagram. Some diagrams, to be sure, are frequently used to accompany the verbal or mathematical description of the model and are sometimes of great help in exhibiting those features of the model that *can* be visualised. However, a typical model of the universe also contains a great many other features that cannot be visualised or pictorially represented, and to become aware of these other features necessarily calls upon our powers of conception or understanding rather than upon our powers of perception. Consider, for example, a typical device made use of in current schemes of cosmology, according to which the galaxies may be regarded as a set of points that trace out a set of geodesic curves in space-time. The general expression for the metric of such a set of points is given by the following formula for an infinitesimal space-time interval (ds):

$$ds^2 = dt^2 - \left(\frac{R^2(t)}{c^2}\right)\left\{\frac{dr^2 + r^2\,d\theta^2 + r^2\sin^2\theta\,d\phi^2}{(1 + kr^2/4)^2}\right\}$$

where $R(t)$ is some arbitrary function of the time (t) and identifies th
expansion or scale factor, k is a constant whose value determi
whether the space is Euclidean or non-Euclidean, c is a constant f
velocity of light, and r, θ, and ϕ are spatial co-ordinates. In
to the specification of purely geometric or kinematic proper
are obtained by introducing appropriate values for t
constant (k) and the expansion factor ($R(t)$) in the ab
model will also normally require some assignment
ical or gravitational properties to the entiti
Additional formulae will be required for
formulae will normally involve the complic
ism of relativity theory or some equivale
obvious that however much a simple d
and lines, or some other pictorial device
representation of what we are talking
encompass all those additional and b

not included in the diagram. These other features will be conveyed by the use of such things as graphs and relevant mathematical formulae, and the total accompanying text of verbal commentary that explicates their meaning and warrant. If we wish to refer to a model of the universe, we must think of all these symbolic devices employed in unison and in inter-relation with one another.

(2) A second elementary point of difference between models of the universe and ordinary physical scale models is also worth noting. This is brought out when we stop to ask what we are to mean by the 'original' for the cosmologist's model. When we reply, 'the universe is the original for his model', it is clear that the cosmologist cannot inspect this 'original' nor can he describe it independently of his model in the same way in which the model builder of a ship can inspect and describe the original of the ship. For the 'original' is not the segment of the universe, the finite population of subsystems which is open to inspection and description. The cosmologist claims to be interested in the universe as a whole, and this is not the same as the actually observable region of galaxies open to exploration by present instrumental resources. Nor again, of course, do we wish to say that the original for the cosmologist's model is some idea of the universe, since the model of the universe, being itself a set of ideas, is not intended as a model of another set of ideas. In the case of an engineer's plan for a ship which he proposes to build, one can speak as we did earlier, of a hysical model that is constructed before the ship itself is built as a del whose 'original' is the idea of the ship. But in the case of the erse, the cosmologist is not undertaking to construct something ot yet in existence. For the universe in some sense surely already he universe is something, whatever it is, that no man ever best, the cosmologist is trying to construct a model of Moreover, in the case of an 'original' that is itself a n inspect and describe the properties of what we are e one who creates an idea or formulates plans, tures he assigns to these plans and ideas. Then is constructed to correspond to these ideas, he or someone else who is familiar with the original plans as well as the physical r, this expedient is not available to the of the existence of t as the Craftsman d ld. aditional th

philosophy that this imagery is employed. And in these schemes of thought, recourse must be had to non-scientific modes of 'knowledge' to assure us, in the first place, *that* such is the case with the universe, even though one might disclaim actual or possible knowledge of *what* God's original plans were or how well His handiwork matches the original. Scientific cosmology is in any case powerless to determine the plan of the universe, if there was a plan in the mind of a Divine Craftsman, or to compare it with the universe as 'created'. What then are we to understand by the 'original' to which we might wish to relate our model of the universe? If it is neither some finite collection of actually observable entities, nor some set of ideas in the mind of a Divine Craftsman, shall we say that the universe is whatever it is whose structure we are trying to determine? But what assurance can we have that there is some entity, here being called 'the universe', that does have some structure antecedently to and independently of our enquiry? However we shall wish to answer this last question, the difference between building a model of the universe and building a model of a ship is obvious. For whatever may be the case with ordinary physical objects of which we make models, or whatever may be the case with a theological or quasi-theological view of the universe, as far as scientific cosmology is concerned, there is no sense in speaking of the universe itself as an 'original' for the cosmologist's model in the sense that this is something that can be independently inspected, described, or reported on in any simple or direct way. Since it mak[es] no sense to speak of the universe as something that can be independe[ntly] inspected, described, or reported on, it follows that it makes no [sense] to look for a relation of possible similarity between model and o[riginal]. Whereas we can compare a scale model and the original to s[ee] extent one matches the other in relevant respects, this [is] not available to the cosmologist. His only access at [all] verse is *through* the model he builds, not *to* the univ[erse] of his model. This means that any simple cor[respondence] of truth is surely inappropriate to our eval[uation] cosmological models.

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NOTES AND COMMENTS

A New Prediction Paradox

A SO-CALLED 'prediction paradox' (or 'pragmatic paradox') was first published by D. J. O'Connor in *Mind*, 1948, **57**, 358 (according to W. W. Quine, *Mind*, 1953, **62**, 65, a similar 'puzzle . . . has had some currency from 1943 onward'). It was discussed in *Mind* by several authors (*Mind*, 1950, **59**, 86, 538; 1951, **60**, 403; 1952, **61**, 265; 1958, **67**, 382; 1959, **68**, 510); Quine, especially, denied that it was a paradox comparable say, to the liar. He thus implicitly denied that it was comparable to Langford's Visiting Card Paradox (see Lewis and Langford, *Symbolic Logic*) which consists of a visiting card on the front of which is written

The assertion written on the other side of this card is true.

while on the back is written

The assertion written on the other side of this card is false.

In view of the controversial character of this prediction paradox, I wish now to describe a *New Prediction Paradox* which, I hope, is clearly analogous to the liar, or to Langford's paradox.

It consists of a card which I recently sent, in an envelope, to my friend K.R.P.:

> Dear K. R. P.,
>
> Kindly put this card in another envelope and send it to our common friend Fred, but only after writing on its back (which I leave empty for the purpose) '*Yes*' or '*No*', according to whether or not you feel justified in predicting that, upon receiving this card, Fred will find that '*No*' has been written on its back.
>
> Yours cordially,
> MARTIN GARDNER

A Comment on the New Prediction Paradox

ng note seems to me to establish what its author claims—a dox closely analogous to Langford's Visiting Card Paradox. rth remarking that it can be easily modified so as to be *gation* (involved in the Liar and in Langford's paradox),

o me, but make sure to write 'Yes', or to put some n the blank rectangle to the left of my signature if, in predicting that, upon its return, I shall find this

Yours sincerely,
K. R. POPPER

DISCUSSIONS

Are Metaphysical Statements Confirmable?

With Mr Watkins's theory of metaphysics [1] I have, on the whole, no real quarrel. I think he is right to take falsifiability rather than verifiability as the distinguishing feature of most empirical and scientific statements, and to use bare assertions of existence—or 'purely existential' statements—as the starting-point for his interpretation of metaphysics. Where I feel uncomfortable about his theory is the further contention that such statements of pure existence, and the metaphysical statements modelled on them, are somehow 'factual' because they can be *confirmed*. This allows Watkins to place them in a sort of 'no-man's land' between analyticity and testability, as the 'logical cousins and nephews' of 'respectable' empirical statements. I think this is a mistake, and I want to show why in the following note.

Suppose someone says 'Centaurs exist'. The claim can hardly be falsified, since we cannot canvass the universe (not to mention future universes) to make sure it contains no centaurs. Yet it is always possible we shall run into centaurs somewhere, sometime: hence it could be confirmed *in some sense*. As far as I can see, this is the nerve of Watkins's argument for the confirmability of metaphysical statements too.

But how do we know a statement like this really is purely existential in the first place? Surely not just by its form: we have to look at the context. If the author of the statement were to add, for example, 'I ought to know centaurs exist—I captured one on the lawn this morning and he's locked in the garage right now', his existential assertion would then become empirical because falsifiable. In many contexts, of course, we could not always be sure: for example, if the author says, 'I ought to kno about centaurs existing—I saw a couple when mountain-climbing in Tibet in 19 The point here is that our judgment of the logical status of an existential stat depends upon what relevant *observational directions* for testing it, if any, are or otherwise provided with it. Only when we are sure none have been s the context may we confidently classify a statement of bare existen existential'.

Assuming now that this statement, 'Centaurs exist', *is* purely a subsequent discovery of centaurs confirm it? In a *formal* sen But without observational directions relevant to testing it, th also be entirely incidental. We could just as well have run a sometime, had it never been made. Scientific and most o on the other hand, have quite another kind of co directions they embody, or which accompany them state of affairs that we might not otherwise hav might be called a *functional* confirmability growin

[1] Cf. 'Between Analytical and Empirical' (P able and influential Metaphysics' (*Mind*, 1958, Empirical?' (this *Journal*, 1960, 10, 287-308)

A REPLY TO DR HUTTEN

Far from being unnoticed, the missing minus sign is immediately recognised by the competent reader as a simple misprint. This is clear from the explanations given in the text; and no error or miscalculation follows from it. Since the date of publication of Dirac's paper, the Klein-Gordon equation must have been printed a few thousand times in papers, books (including that of Dirac), and even textbooks. Except for the occasional misprint, the equation is correctly reproduced everywhere.

The author's insinuation that he has discovered an important mistake is therefore totally without foundation. For that matter, the formulae Mr Hanson quotes in his own paper contain many mistakes; but I take it they are merely misprints. It is surely not possible to write the history of scientific discovery on the basis of the discovery of misprints.

ERNEST H. HUTTEN

A REPLY TO DR HUTTEN

IN the footnote which has displeased Dr Hutten,[1] there occurs not one literal untruth. There I state that on page 611 of Dirac's 1928 paper the Klein-Gordon equation is improperly written. It certainly *is* improperly written. I say also that equations (1) and (3) in the 1928 paper are inconsistent—and they *are* formally inconsistent. I state also that this fact has not been signalled during the last thirty-three years, and that Professor Dirac has confirmed my suggested emendation of (1). Both of these contentions are also quite true.

Since the footnote's factual content contains not one falsehood, Dr Hutten's dissatisfaction can only be expressed as he does express it, viz. that I have *insinuated* that I have made some important discovery. I can find no basis whatever for that interpretation. I certainly do not *say* that this is an important discovery of a mistake; all that I say is set out in the first paragraph above. In fact, in my very first letter to Professor Dirac in which I call his attention to this error, I write '. . . the slip is not [illegible] important one, typographical at most. Since equation (3) does have the correct [illegible] everything from that point on follows easily.' (Letter of 24 March, 1961.) Professor Dirac himself replied 'There *is* a mistake in sign in my 1928 paper, [illegible]oint out . . . I do not remember it having been pointed out previously.' [illegible]0 March, 1961.)

[illegible]t but agree with Dr Hutten that the discovery of misprints is hardly [illegible]s on the history of science. That is precisely why my 'discovery' [illegible]e to my paper, rather than as a substantive part of my argument. [illegible]anything in my form of words that could justifiably have led [illegible]s 'insinuating' something, then I can do more than offer an [illegible]st insist that there is nothing whatever in the words I [illegible]Jutten's own insinuations. Finally, I can see no virtue [illegible]in 'one of the greatest papers of the century' (Bethe's [illegible]diately recognised by the competent reader'. There [illegible]editions of Newton's *Principia*, a fact which scholars [illegible]re working hard to emend.

NORWOOD RUSSELL HANSON

[illegible]961, **12**, 205, n. 1

REVIEWS

PSYCHOANALYSIS AND SCIENTIFIC METHOD

IN 1958, the New York Institute of Philosophy held a symposium which discussed psychoanalysis, philosophy, and scientific method. A book containing the contributions of twenty-eight of the symposiasts, two of them analysts and eighteen of them philosophers, came out in the following year.[1] It has a section devoted to the bearing of psychoanalysis upon the social sciences, especially sociology. It has a section based upon a paper by Morris Lazerowitz, in which he gives an account of the rôle of unconscious fantasy in the making of philosophical theories, using Bradley as his principal example. I found this section confused because Lazerowitz did not make it clear whether he believed :

(*a*) That philosophical theories, no matter what other interest they have (and they *do* possess *bona fide* philosophical interest), throw interesting light upon the unconscious minds of their creators, either by way of providing clues, or by acting as a mirror to the creator's unconscious processes ;

(*b*) This version extended to all who do philosophy—a kind of people who are ' born ' empiricists or idealists, ' born ' supporters of objectivity or relativity in ethics—in the way in which people are sometimes said to be ' born ' Conservatives or Anarchists.

Or :

(*c*) Whether he believes that philosophical systems have *no* intrinsic s
justification—philosophical propositions only acquiring meaning
viewed as imparting certain facts about the proposer's unconscious
fantasies, etc.

Or :

(*d*) Whether he thought that this is only the case wit
theories. Just as on the conscious level we start imputing
a medical explanation, if an otherwise sensible fellow
the patently indefensible, so the philosophical systems
chaps like Bradley and Spinoza can only be explai
by relating them as effects of certain causal facto
unconscious minds. This view involves a ju
Bradley's and Spinoza's philosophising, and a

I suspect that Lazerowitz was maintai

[1] *Psychoanalysis, Scientific Method and Philosoph*
Press, New York, 1959. Pp. 370

> the difference between the theories might be phrased like this : According to Freud, there is a ' disease ' which produces symptoms ; cure the disease, and the symptoms will vanish. According to the alternative view, there is no ' disease ' ; there are merely wrong habits which have been learned and must be unlearned. If such ' unlearning ' and ' relearning ' is efficacious, and there is no evidence of any ' disease ', then surely we must dismiss this additional concept as superfluous.

And, next page, ' even a single case of this kind, viz. removal of a symptom, with no following process of substitution, by non-analytic methods, is crucial, because on the Freudian theory such an outcome would be impossible, and one positive case can infirm a theory which denies the possibility of such a case existing.' This is not the place to discuss the validity of this literature and these studies, but philosophers and psychologists must see that they cannot *both* assert the unscientific irrefutability of psychoanalysis, complain that they cannot even *understand* what some psychoanalytic propositions can mean, and *also* produce evidence in the belief that they are refuting these hypotheses.

Nagel thinks that ' there is an important failure of analogy between conscious motives and unconscious mental processes ', because

> these unconscious motives have an enduring character and tenacious attachment to specific objectives that conscious wishes do not exhibit. (Thus) a thwarted wish of early childhood . . . may not completely vanish but may enjoy a repressed existence in the unconscious and continue to operate in identical form into the present (p. 45).

I do not really see why a thwarted childhood wish should not survive on conscious or preconscious level for a very long time, without changing rm, or losing its strength. What are the searches for the philosopher's he elixir of eternal life, the cure of cancer, but dramatised examples of ishes on the part of individuals? What of the wishes for a life travel, to become a musician, to acquire a good education, person might nourish from childhood or adolescence, and fy, for one reason or another?

real, no less conscious/unconscious, for being precisely ed or focused, and it may persist on the conscious Of course, conscious wishes are usually satisfied, or —though this can prove impossible—whereas unrenounced—until they are first made conscious. Nagel meant to say. But there are supposed conscious and unconscious processes! It is no is a one-one relationship between the structconscious minds—but rather that in some

respects they are similar but in others they are very different. However even if the differences adumbrated by Nagel between conscious and unconscious mental processes were as he describes, they would not appear to add up to ' an important failure of analogy '.

Nagel's second conceptual difficulty is with mental causes. ' As to the notions of unconscious psychic processes possessing causal efficacies—of unconscious, causally operative motives and wishes that are not somatic dispositions and activities— . . . I must admit that such locutions are just nonsense to me ' (p. 47). It is rather difficult to discover just why Nagel thinks them nonsense. It is not as though we had settled the question as to how *conscious* wishes and motives can be said to cause the actions and fantasies which are their result. In general, the more we can keep the notion ' cause ' out of our account of wishes, motives, intentions, the happier we are. The same applies, *pari passu*, to talk of unconscious causes and to the ghosts in the machine and the anthropomorphising of unconscious mental processes, imputed to analytic writers. These are pieces of philosophers' lumber, trustingly accepted by psychoanalytic writers, but in no sense essential to their work.

On the other hand, Nagel frequently speaks as though *philosophers* had no problems left in philosophical psychology—as though we had general agreement on questions such as the precise relationship between ' the theoretical mental apparatus and . . . the somatic mechanisms and capacities of the human organism ' (p. 47). Thus Freud is criticised for asserting that ' psychic reality is a special form of existence which must not be confounded with material reality '. This was the kind of remark being made by philosophers not so long ago. Anyway it *is* the case that we do not use ' mind ' and ' brain ' interchangeably.

Then there are the troubles which arise when one tries to define ' cu
Scriven thinks that ' to listen to the analysts one would suppose they
little agreement about the goals of therapy, i.e. the definition of
After quoting a number of proffered definitions, he concludes : '
of these diverse suggestions, how can we ever test *the* proc
analysis?' (p. 241).

Definitions of cure (or of anything) need not rule
court, though they in fact may do so. We have to see
any two criteria are incompatible, and if so, in whi
criteria mentioned by Scriven are not incompatibl
different parts of the curative process. (I will ex

Criteria employed by psychoanalysts are n
rather complement or amend previous ones
from a concentration by an analyst upon a pa
a stage in the curative process, which for o
be especially important. The fact that h

tell us how many of these people would have recovered to the same extent, given the same lapse of time, without treatment'. The statistical materi.l is indeed 'unsatisfactory'. The differences in result between one study and another make them very nearly worthless as arguments against analysis or any other form of therapy. Further, surely any psychoanalyst would simply deny the possibility of many of the people whom he treats improving, let alone recovering, spontaneously. One would also expect surgeons who perform prefrontal lobotomies to doubt whether many of their patients could have spontaneously, and permanently, recovered. Their activities would appear somewhat reckless otherwise.

The theory of psychoanalysis provides a detailed account of character structure. It seeks to explain how different character structures and character components form and change, or fail to change. It employs a system of classification of mental diseases which draws upon classical psychiatry, and which it seeks to relate to its character typology. To try to isolate the mental disease or systematic psychic malfunctioning from the character structure of the sufferer, that is, from his overall personality, is an enterprise incompatible with a belief in psychoanalytic theory. What is more, it is one in which we do not engage in everyday life. To do so involves either a curious compartmentalising of the psyche—a new kind of faculty-psychology—or else the subsuming of mental diseases under 'bad habits' of a purely physical kind, if, that is, one can really make sense of such a notion.

For, irrespective of diagnostic disputes between psychiatrists which are often over where to draw the line, it is generally accepted that many persons are markedly schizophrenic, or manic depressive, or just neurotic, that they did not get this way overnight and that the alteration of such character structures is not an overnight matter either. Thus one does not wake up morning a schizophrenic, or come home to find one's room-mate has a manic depressive in one's absence. This is not to say that the symptoms may not be dramatically sudden—or that many florid of the most serious mental disturbances may not disappear It is just that we always try to relate neurotic and psychotic to a person's past life, which we treat as one of a piece. choses or neuroses just happen, and just 'go' is to fall session or Acts of God. The suggestion that schizo- encies, are just bad habits is a staggering under- iouristic rhodomontade.

ny people talk of mental disease—as though it les, instead of something more like a serious t of muscles atrophied after polio, or even kind 'remit spontaneously' we talk of o occur at Lourdes.

It is as though Professor Eysenck's 'ethnocentric, prejudiced, authoritarian personality' which he constructs from a significant cluster of attitudes and predilections, fantasies and prejudices, ranging from anti-Semitism, firm views on capital and corporal punishment, to phobias about germs, were to turn up at the next survey, voting Liberal, preparing to marry his Jewish fiancée in a Liberal synagogue, supporting bodies in opposition to capital punishment and fox hunting (but not too shrilly), espousing the educational views of A. S. Neill (while pointing out the difficulties), and so on. Eysenck's and Adorno's theory would be as ill prepared for this spontaneous remission of authoritarianism as is Freud's for a similar metamorphosis of paranoid schizophrenia or obsessional neurosis.

In the same way, a great deal of the world's literature and drama would be rendered meaningless if the Borkman's and Lady Macbeth's, the Don Quixote's and the Mr Barrett's were to take on, or shed, their megalomanias, their insane greeds and incestuous jealousies, like a change of clothes. Not only analysts, but moral, religious, and political reformers, would seriously doubt the chances of a person whose life was tormented by envy of every creative or hopeful person around him, or jealous of every relationship which his friends or loved ones enter into, throwing off his destructiveness and despair like a rather bad cold. *Of course* people see the light, and are converted, and sign the pledge, but we watch them suspiciously for a long time afterwards, keep the key to the liquor cupboard, and remember that Scrooge came out of a Christmas tale, and that many have serious doubts about St Paul.

If it is said that politics, literature, and our everyday experience of people is one thing, and mental disease or illness another, that mysterious sea changes are not to be expected in the former (though they *may* occur), but occ frequently (perhaps as a rule), in the field of psychic ill health, then psyche must be a very curious thing. The philosophers seem prepa give a dispositional account of a normal or healthy personality, buta to do the same for the behaviour patterns of the sick, whose obsessions, systematic dreamwork, and repetitive and monoto anxieties, have to be seen as a vast concatenation of separat

One has a lurking suspicion that some symposiasts bel 'mental illness' is a misnomer, a paradox, and that electrotherapy are the only conceptually valid mo flushes the obsessions out of the brain, not the n removes the delusions and anxieties along with

In conclusion, the major contributions of t suffered from two important defects. Firstly handled with comparative unfamiliarity and expressions like 'mythology', 'religion 'the vicarious professional imperialism o

socialists and 'interventionists'. The author insists that any interference with the free price mechanism can be based only on confusion of thought and can lead only to economic disaster.

> No logical or scientific arguments whatever could or can be brought against the theories of liberalism which were developed by the philosophers, economists and praxeologists of the eighteenth and of the first half of the nineteenth centuries. Whoever wishes to combat these doctrines has no other means available than to dethrone logic and science by attacking their claim to establish universally valid propositions (p. 189). . . . To the man who adopts the scientific method in reflecting upon the problems of human action, liberalism must appear as the only policy that can lead to lasting well-being for himself . . . and, indeed, for all others as well. Only one who does not want to achieve such ends as life, health, and prosperity, . . . only one who prefers sickness, misery, and suffering may reject the reasoning of liberalism on the ground that it is not neutral with regard to value-judgments (p. 39).

Little argument is presented in favour of these assertions in the present book, although more may be found elsewhere in the author's writings.[1] The main argument appears to be that a rational use of economic resources must depend on a free market mechanism for the pricing of factors of production. Von Mises indeed performed a most useful service in forcing economists to consider the problems of production and distribution in a society where the factors of production were in public ownership. Although a great deal has been written on this problem in the last thirty years, however, some of it in direct reply,[2] this new translation contains not even a footnote in reference to the discussion.

It is a paradox that although writers of this Liberal School have performed useful service in exposing the Hegelianism implicit in such notions as 'the ial welfare', they have sometimes become a little Hegelian themselves. oint of the advocacy of the freest possible trade is that it is considered imise the national income. The mere size of the national income, considered in abstraction from its distribution, is only of academic ny individual. Even if we consider 'such ends as life, health ' in solely material terms, it is possible for proportionately ve more of such ends in a poorer country than in a richer.[3] e other writers of this persuasion, von Mises makes no re' that may be derivable from being a member of a ciety, as such.) Nor does he treat seriously the

ialism, New York, 1937, and 'Economic Calculation in F.A. von Hayek, (Ed.) *Collectivist Economic Planning*,

ppincott, (Ed.) *On the Economic Theory of Socialism*, cited by Thomas Marschak, 'Centralization and zations', *Econometrica*, July 1959

ction, op. cit., pp. 674-675

problems raised by dynamic economic change, and by immobility in face of it. He writes,

> The defenders of interventionism have occasionally attempted to confront the arguments of the critics of this policy—arguments supported by the irrefutable deductions of economics—with the alleged fact that the propositions of economics hold true only in the long run (p. 162),

but leaves it at that, which is a pity; economic liberalism can often make an attractive case for itself precisely by arguing that some interferences with market forces serve only to retard the attainment of a new equilibrium and, in effect, to prolong the agony of the groups on whom change is bearing most harshly.

The unrevised publication of this book, much of it concerned with ephemeral controversies in the German tongue, is a little surprising. It has the same pugnacity as *Human Action*, but pared down to the bare bones of dogma with none of the often brilliant supporting argument of the earlier book; it suffers also from inelegance of translation. No work on methodology can be above suspicion, even when it is clear, if it dispenses with examples from the discipline concerned, and to present the rather dubious epistemology with none of the economics can do no service to its author's reputation. If the William Volker Fund considers it useful to revive these me[illegible]war *à l'outrance* it might serve its purpose better to translate sc[illegible]Russian texts on Marxism, to serve as a permanent inoculation ag[illegible]this sort of obscurantism. One can only assume that the present object is to lay a smokescreen of verbiage that will discourage students of economics from the serious appraisal of economic arrangements to which they are naturally prone.

ANNE MARTI[illegible]

ON BROAD'S CONTRIBUTION TO PHILOSO[illegible]

THE contributors to the present volume[1] almost certainly [illegible] well-known who has been deeply influenced by Broa[illegible] a pupil of his, or who has made a special study of h[illegible] impossible to find, for purposes of reviewing it, son[illegible] a contributor. I wish myself to write this review[illegible] about Professor Broad that I wish to say, but I [illegible] remarking that after graduating, when I spen[illegible] Broad was lecturing only on subjects that h[illegible]

[1] Ed. by Paul A. Schilpp, *The Philosophy of C.*[illegible] York, 1959. Pp. xii + 866

own background, so for this reason and this reason alone, I attended none of his lectures. I therefore hardly came across him, and indeed have only met him on very rare occasions since. My appreciation of Broad's philosophical endeavour is therefore in no way biased by personal contact. On the other hand, I am remote from one influential contemporary view that Broad's approach to philosophy is completely misdirected. This at least gives me the opportunity, if I can make use of it, of writing a fair appraisal of his work. It is permissible, moreover, to attempt to do this, because in fact Broad himself with great candour has not only recognised the limitations of his own work but he has said publicly what they are. Beyond doing respectable work he does not think that he has made any outstanding contribution and he considered long ago that he had nothing further to offer. It should perhaps be added that there is not a trace of suggestion that he is asking for anyone to turn round and say, 'nonsense, old chap'. He has obviously come to terms with what he conceives to be his limitations in a realistic way and he is not going to be hurt if others accept his valuation.

(1) It would be hard to deny that Broad has done respectable work—'respectable' meaning painstaking, clear, conscientious, useful in some measure, directed towards something that has some degree of significance. It would be hard to deny that he has done an enormous amount of it. But is he right in claiming that he has done no more than that? One can see his point. His merit has lain in great clarity of writing, dissection of problems in such a way that one can see the wood for the trees and tackle aspects of a problem without getting them all mixed up, and perhaps most important, in his dissection of arguments on a level of high grade commonsense. This means that while one might not look to Broad for inspiration one would be very foolish, on beginning to investigate a subject, if one did not consult what he had to say about arguments, ideas, and problems in the field. Under this heading one might regard Broad as an outstanding teacher, not of undergraduates and graduates, but of colleagues. I wish to stress this aspect of his influence upon clarity is almost certain to become completely unnoticed. Bernard Shaw, to use a debatable analogue, may become forgotten as a social reformer simply because his main ideas have been absorbed and accepted. Likewise, if philosophers now embody Broad's standards of clarity they will forget what the scene was like when Broad tried to introduce them. It is enough to say that he wrote against a background not of obscurity that can be said to be understandable but difficult, but against a background, a woolly and unintelligible form of Hegelianism in this country. It is not too much to say that he was unique, and Broad effected the revolution in clarity. Moreover, Broad cannot owe much of his achievement to the philosophical world of the day, apart from his teachers

This amounts to a contribution that goes beyond tidying up, adding minor adjustments, and making criticisms that leave matters essentially where they were: he played a notable part in a revolution (of the James II-William III type rather than Cromwellian). This is more than respectable.

(2) Such a contribution is nonetheless compatible with a dearth of the highest kind of originality. In claiming to have done no more than respectable work, Broad is saying that he has made no positive contribution in the sense of solving a problem however small or putting forward an original idea that might be of significance. But let us see whether this is literally true. He provided, for example, an analysis of the concepts of substance and process which Professor L. J. Russell, writing in the present volume, says constitutes a unique example of philosophical analysis, and readers of this *Journal* will probably know that Professor Russell is an excellent judge. This particular sample is not likely to be well-known because it occurs in a work of Broad's that will seldom be widely consulted, because the subject of its enquiry was McTaggart, who one might say, was a kind of Spinoza born 200 years too late to occupy a place on the philosophical stage. Under the general heading of Reductionism, one can see many problems in perspective in modern times. Dedekind reduced irrationals to rationals, Russell reduced numbers to classes, phenomenalists tried to reduce physical objects, like tables, to sense-data, Whitehead reduced points to classes of spheres or classes of boxes. Now there was a problem about the nature of propositions (as opposed to statements and sentences) and Broad provided a reduction in this important tradition (important even if it is wrong) by reducing the proposition to a class of judgments. In his work on induction he introduced an original idea, as did Keynes, to try to solve the problem by means considerations of probability and limitation of variety. I do not mean this was the right answer, in fact I think it was a failure, but the great philosophical ideas usually has little to do with whether or not the turn out to be true, and I personally think that the Keynes-Broad of the most ingenious of modern times, even though it seems t no future in it at all. (Incidentally Broad did the work m Keynes; Keynes probably originated the idea; but both immense debt to W. E. Johnson). These are the onl me of something creative, but I am not familiar Does it not in sum perhaps just cross the boun original work in the chief sense?

(3) Broad was not averse to utilising know he could lay hands on, if he thought he coul sensory perception, for example, he would do as a great supporter of this subject, but I thi he is the supporter of research in it; I do

milieu; but when he adopted these practices they may have looked counter-conformist. Nonetheless, such practices would not, like new ideas, cause shock; and it seems possible that Broad did not have it in him to throw over the traces and be ready to shock the accepted sense of good form in philosophy.

I have written at this length about Professor Broad because readers of this *Journal*, I suspect, know too little about him. Pure philosophers will know about him, but the great majority who belong to the school of predominant influence will have forgotten him or will ignore him. I feel that philosophers of science should at least have the opportunity of having their attention directed to Broad's work. Though they may not always get inspiration they will find a conscientious colleague usually abler than themselves, and this is not to be despised. I do not think that Broad should be on the shelf to the extent that he is (and I doubt if the strong defence I have given in (1) will make much impression with those who do not know what the general standard of philosophising was like); but it is a matter of some consolation that he himself accepts the position without resentment, even with satisfaction.

J. O. WISDOM

Discourse on Method. By René Descartes. A New Translation by Arthur Wollaston.
Penguin Books, Harmondsworth. Pp. 192. 3s. 6d.

THE volume contains a new translation of the *Discourse*, the *Meditations*, and the letter preface to the French Text of the *Principles*, together with an troduction of twenty-six pages, summaries to each section, and two pages otes on Cartesian Terminology by the translator.

the intention of the publisher is to add a translation of Descartes that ble to the list of Classics being made available to a very general the intention is satisfied. The text reads easily, and should make or a large number of people to discover how easy it is to read

g Descartes is, of course, a different matter, and this the otes are designed to aid. Whether any brief study can er very much may be doubted; but my concern is o tend more and more to collect paper-backs as their who have to take this into account. A satisfactory use.

on states simply (p. 31) that the translation *hod*, etc., giving no indication of which texts has taken pains to give details of Descartes's Introduction, details which (however little

they concern the general reader) are often surprising and misleading. Descartes is said (p. 28) to have 'turned his attention once again to the common reader who had welcomed the *Discourse* and the *Essays*' because the *Principles* in 1644 met with no more success than had the *Meditations*. 'The *Meditations* were soon put into French by the Duc de Luynes. Descartes then published a second edition of the *Meditations*, which included the objections he had solicited from various men of learning together with his own replies, and he requested the Duc de Luynes to include them himself in a second edition of the work which Descartes had read, and on which he had set the seal of his approval. Then, in 1647, came a French translation of the Principles. . . .'

No mention is made of the anonymous publication of the *Discourse*; evidence for the 'common readers' being enthusiastic, about the *Essays* particularly, would be welcome (see below); the first edition of the *Meditations* was in 1641 (Paris) and was followed by a second in 1642 (Holland); no evidence is given for rejecting the accepted view, which is in fact a statement about the Soly edition, that six sets of Objections and Replies were included in the first edition, the seventh appearing in the second; Mersenne did almost all the soliciting (and in so doing gave Descartes much irritation and labour, and must almost have wrecked the naively ingenious scheme to ensure approval by the learned authorities); I know of no other suggestion that a French version appeared before 1647, and would be pleased to hear of any evidence for it, as I would for what is implied, that Descartes asked the Duc de Luynes to translate the *Meditations*, or that Clerselier's translation of the *Objections* and *Replies* was not an independent undertaking. And even if the Duc de Luynes completed the translation in 1642, Descartes's notion of priorities is indicated by the revision and publication of the *Latin version* of the *Discourse* in 1644, while in the preface to the *Meditations* he insists that questions of God and the Human Soul are merely touched on in the *Discourse*, it being inexpedient to discuss them at length in French and in a 'discourse which might be read by everyone'.

Translators in the past have suffered from the existence of [illegible] French versions, though we find Veitch using an original text with inclusion of variations in the approved translation, a proc[illegible] much is to be said. The serious student, and his teacher, [illegible] text is in question, and if (as they should to underst[illegible] Descartes himself demands in the preface to the Soly[illegible] *Objections* and *Replies*, it must be the Latin text. [illegible] a version that is neither of Descartes's, and justi[illegible] (cf. the difficulties of Haldane and Ross). Tha[illegible] both texts can be established only by compari[illegible] crucial point is that we have to do this all t[illegible] sections that concern me for special reasons[illegible]

of Meditation III that the French version of the expansion of '*res cogitans, id est . . .*' includes '*qui aime, qui hait*' (easy to accept of Descartes, not so easy of a soul), and Mr Wollaston, omitting these, appears to work from the Latin; yet the next sentence translates '*en elles-mêmes*', which has no Latin counterpart.

On p. 113 we have 'But what precisely do I picture to myself when I think of it in this way?', which is the French's '*Mais qu'est-ce que précisément parlant que j'imagine lorsque je la conçois en cette sorte?*', not the Latin's '*Quod est autem hoc praecise quod sic imaginor?*'. Yet in the same paragraph Mr Wollaston avoids the French version's '*infinité de semblables changements*', and renders the Latin's '*innumerabilium eius modi mutationem*' by 'a countless series of shapes.' (The first sentence of the next paragraph is mistranslated from the Latin. '*Quid extensum?*' cannot be given as 'What now is extension?'; what succeeds it becomes a contradiction of a central Cartesian principle, and nonsense.)

If we turn to a passage in the *Discourse* which concerns me for special reasons, we find for the fourth rule (p. 50) 'to make so complete an enumeration of the links in an argument, and to pass them all so thoroughly under review, that I could be sure I had missed nothing'. The French (and original) reads: *de faire partout des dénombrements si entiers et des revues si générales que je fusse assuré de ne rien omettre.* The translation is interpretative, and a glance at the Latin (the title page of which refers to emendations by the author) or at Gilson would have saved interpretation from being misleading. The question is not simply one of intuiting the links of a constructed argument, but also of ensuring completeness in the division of a problem into its unanalysable constituents and in the bringing to bear of all relevant knowledge upon the clarified issue.

Surely, on general grounds it is best to use footnotes if explanation is felt ...essary? True, nothing of this need concern the general reader. But it ...s that, for student or teacher once again, the whole text must be checked.

...*troduction:* An attempt has been made to include a great deal of ... the twenty-six pages. Ordinarily it would be pointless to ...about omissions and inclusions, since selection is emphasis ...ew various; but the emphases here are disturbing, incon- ...nction with errors as to fact, misleading.

... on Descartes's lack of desire to publish, after an initial ... looked askance at what he called his son's propensity ...ound in calf", that is, for publishing tomes of ... father recorded such a view, which I have not ... died Descartes had published only the ... Mr Wollaston does

scholarly activities. But the evidence, and the account given here of Descartes's aims, is against his having one, unchanging attitude to publication.

Descartes's own estimate of the *Discourse* (as not a treatise, as an introduction to what was important) is not mentioned, and the effect on 'the ordinary cultivated man and woman' (p. 12) and the 'common reader' (p. 28) is heavily emphasised. So, too, on page 17, we find '... the educated lay public for whom the whole volume was primarily intended responded heartily; but academic circles, in particular Descartes's old masters, the Jesuits, whose approbation he felt would have won him the battle, remained obdurate in their rejection of his anti-Aristotelianism'. Here 'lay *v* academic' is misleading for the period—the Orders were not included in, nor coextensive with, the Universities, and to suggest that the Jesuits as such were simply 'Aristotelian' *dévots*, that members of the Society of Jesus, of the Oratory, and the Jansenist association, were agreed in these matters, is to ignore history. Further, 'for whom the whole volume was written' is certainly wrong on the score of the *Géométrie* at least; there were difficulties over the Chancellor's 'privilege', but these were of Descartes's making; and Descartes's letter to Mersenne of 9 January 1639 (to which Roth has drawn clear attention) states that Mersenne's trouble taken over printing errors is to no purpose, since the bookseller had sold so few copies that Descartes saw no likelihood of its being reprinted—which goes flat against the claim as to the attitude of both 'lay' and 'academic'.

On page 10 Ayer is quoted misleadingly; the context suggests that his position has kinship with the notion of philosophy as 'an autonomous form of knowledge', as having 'a beginning self-assured', carrying 'its own credentials of truth'; and further he is misquoted. I suspect a misquoting of Father Daniel (p. 25), a phrase misplaced, though I cannot check it here and Roth may have erred. But the force of the passage is misunderstoo and the section revealing.

Roth quotes at length, and concludes with the passage in que He wants to stress Daniel's picture of the widespread enthusi *Cartesianism* outside 'almost all' the *Bodies* and the *Universities*, of t tion between the *Old Philosophy* and the *New Philosophy*. states that 'Cartesian' is a label for any novelty in Natu with no question asked of relation to Cartesian doctrine. sandwiches the passage between 'The vogue of Cartesian than a generation' and 'Such a tribute coming from but thirty years later it was a different story. published his Mathematical Principles of Natural clear that the Cartesian system of the physical u past, at least among serious scientists'.

By hein

Schrödinger's sympathetic insight into an intellectual situation at some point in history in fact contrasts with the occasionally 'anti-philosophic' attitude exhibited by Boltzmann during the latter's brief tenure of a professorship of philosophy (though the emotional attitude of Boltzmann may perhaps be partly explained by his exasperation in defending atomism against Mach).

The inaugural address 'What is a Law of Nature?' is remarkable for its date. In 1922 (five years before Heisenberg enunciated the uncertainty principle) Schrödinger argued a case in favour of indeterminism, based on Exner's arguments (which, incidentally, date back to at least 1908, not merely to 1919 as stated by Schrödinger).

This illustrates the complex character of Schrödinger's views, which are well-known to have been 'anti-orthodox' in later years; one of the features of this (present) 'orthodox interpretation' of quantum mechanics is the implication that we have, in a certain sense, reached an absolute end to the parametric analysis of the behaviour of systems (e.g. analysis either in terms of location or in terms of momentum). An opponent of this view might still allow ultimately the operation of the laws of chance (as does present theory), but consider the present 'observables' themselves the result of, say, Brownian motion of a finer substratum. These quarrels about 'interpretation' would, in part at least, be more correctly called quarrels in prophecy of future developments of theory.

While Schrödinger's occasional downright partisanship of the wave-model (as against the particle model) was perhaps surprising, he never undervalued the arguments against his point of view. Indeed, his every sentence has to be read carefully, whether summarising the arguments of opponents or his own view. There is solid technical backing to his most hand remarks. In his popular writings his omission of explicit reference ch technical justification occasionally amounts to a fault. His own egins to emerge from a study of several essays taken together. It is nple one—witness this championship of acausal nature on the one on the other his reproach to modern physicists for happily permanent a limitation on the description of nature.

d remarkable feature of this book is the subject matter of the ch might be referred to as 'the non-individuality of particles'. uch-discussed revolutions in physics epitomised by the nd the Heisenberg uncertainty principle this constitutes physics, which has perhaps even more direct bearing less widely discussed or understood. It stole upon ssential technical step ('Bose' statistics) was 1900.

he 1925 Heisenberg revolution as the death of o this development as the death of individu- n of atomic substance pushed to its ultimate

extreme, and, at the same time, the elimination of a cardinal feature of the classical atom.

The subject is frequently misrepresented even technically in widely recommended standard textbooks of quantum mechanics.

Whilst the particles of a given type (say, 'helium-4' atoms) were, of course, taken to be *indistinguishable* already in classical physics (e.g. by Boltzmann) we now assume particles to be not only indistinguishable, but, in fact, to be without individuality. This is an additional assumption, leading to new physical results not only relating to statistics.

This non-individuality of particles is *not* derivable from the Heisenberg uncertainty principle. It is true that the concept of individuality is closely linked to the requirement of continuity. But the idea of a continuous path of a particle is only denied *exactness* by Heisenberg's principle. We can devise thought experiments in which the path is sufficiently well defined to make the effect of overlap of wave-functions negligible, and in which nevertheless the case of non-individual particles differs from that of Boltzmann particles. We thus show that the uncertainty principle is neither necessary nor sufficient to account for non-individuality. The latter has to be introduced as a separate axiom.

Furthermore, even non-individuality does not lead necessarily either to 'total symmetry' or 'total anti-symmetry' (corresponding to Bose and Fermi statistics respectively) of the wave function in first quantisation. Indistinguishability is automatically ensured by the symmetry of any operators corresponding to physical observables, and puts no restriction whatever on the 'state' of the particle system. It would be well worth while dis-
cussing conditions on which the preferred status of 'bosons' and 'fermion
might be based.

The topology of the problem of non-individuality includes G
paradox and the point, repeatedly emphasised by Schrödinger, that q
mechanics is, in a sense, never speaking of single particles at all.

In this chapter we have one of the few attempts to indicate the p
interest of the subject.

It is a common (though refutable) attitude amongst na
to deny the influence of philosophy in the production
Schrödinger, for one, was able to see the hen for the egg

The Major Achievements of Science. By A. E. E. M
Cambridge University Press, London, 196
Vol. II. Pp. 195. 17s. 6d.

THE title of this book gives an excellent idea
McKenzie has done is to give an account o

the natural sciences since the Renaissance. His canvas is a wide one. For not only does he explain the actual scientific discoveries in physics, chemistry, astronomy, biology, and medicine—though not mathematics—but he also finds space for biographical details about the great scientists, general chapters about the cultural background of the different centuries, some discussion of the relations between science and technology, and some short and sensible remarks about scientific method. The first volume contains the historical account; the second is a collection of ninety-one extracts which have been chosen and arranged so as to supplement the relevant chapters of the first volume.

Here, as with every history of science, each reviewer will have his own minor criticisms. Individual statements will seem misleading—e.g. 'Aristotle's science was a logical system in which the truths of nature were exhibited as deductions from ultimate, universal principles.' The exposition of some topics—for example, in my case, that on relativity—will seem to some to be overconcise or unnecessarily difficult. Each reader will consider some of the extracts too obscure—those from Kant and Demos occur to me—and others less appropriate than possible alternatives. But these minor criticisms are comparatively unimportant. For, in general, the book is extremely well done. The narrative is clear and concise—though I am not convinced that the author was right to avoid all mathematical formulae; the extracts are interesting and well-chosen. It is difficult to see how it would have been possible to give a more comprehensive and accurate survey of this period of science in a book of this length.

But unfortunately these virtues of succinctness and comprehensiveness from another point of view, defects. For Mr McKenzie makes it clear s preface that one of his main intentions was to provide a textbook could be used in teaching the history of science to arts specialists in forms of schools. Now certainly a brilliant teacher could use this asis for a successful course; but for most teachers and most forms much too difficult. Mr McKenzie's one-page summary of *The e Reason* is excellent, but what is a boy of sixteen going to make how can he be expected to understand the following passage

> ting a model as an ultimate explanation, anticipated the modern physical theories. A physical theory does not describe or ture of the world of sense observation. It merely depicts usually mathematically, functional relations between rests upon its success in making predictions about

itious to attempt to cover so much ground in foolish to take the whole of English history ne going to teach general history to a group I feel that most students—and this would

apply just as much to first-year undergraduates as to sixth-form schoolboys —would benefit much more from a more discursive and more limited approach such as one gets in books such as Butterfield's *Origins of Modern Science*, Conant's *Science and Common Sense*, or Toulmin and Goodfield's *The Fabric of the Heavens*.

But it is, I think, worth posing an even more fundamental question: Is the history of science the best subject for getting arts specialists to understand something about science, the best subject for bridging the gap between the 'two cultures'? At first sight it has an obvious claim, for the very name suggests a discipline with one foot on either side of the gap. And this is not all. Many scholars—including scientists, historians, theologians, and students of literature—have found it a fascinating field. For, more than perhaps any other subject, it relates different aspects of history. One is able to see the connections between scientific discoveries and philosophical or theological presuppositions; one can see the relations between scientific or technological developments and the economic and social background; one can discover how scientific ideas have fired the imaginations of poets and at the same time have given rise to new philosophical theories about the world.

The fact that the history of science is a stimulating subject for mature minds does not, however, necessarily imply that it is suitable for sixth-form schoolboys. The establishment of connections is exciting if one is already familiar with the two things being connected; it can be interesting if one already knows about one of the things; but the significance will be entirely lost if one is being shown for the first time not just the connection but also the things being connected. And the schoolboy will only too often be i this position. He will know little about the Renaissance or about eighteen century Deism or about English Empiricism or even about Fundament or the idea of *laissez-faire*, and so for him the connections so careful plained will most often appear unilluminating and artificial.

Certainly it is desirable that sixth-form boys specialising in a should be taught something about science. This is not because th of facts about science that every educated person should kn fostered by radio programmes and school general knowl produced at Christmas-time by some national newspap one. What is important is that everyone who emerges system should have, firstly, a genuine curiosity abo some understanding of what the scientist is doi the motives which inspire the scientist in his w aims may be realised by courses in the histor that they are far more likely to be met by a c science, a course which goes right up to the w Such courses would, naturally, have to be r have to omit the mathematical foundation

to construct such courses on, say, modern astronomy, or on evolution and genetics, or even, perhaps on modern atomic theory, courses which would introduce the pupils to new facts and phenomena, which would enable them to appreciate that there are still some issues which are unresolved, and which might make them genuinely curious about the results of future research.

If such courses were provided there would still be a place for courses in the history of science and for such books as Mr McKenzie's; for they would be valuable not only in the second and third years of university arts courses but also for giving *science* specialists, both at school and at university, some understanding of the historical background to their disciplines. What I am arguing is that the history of science is not the best way of awakening an interest in science in students who have only too often at the age of 15 or 16 given up science with a sense of relief at the thought that now they could specialise in arts subjects without having to struggle any longer with chemical formulae or mathematical equations.

H. G. ALEXANDER

Measurement: Definitions and Theories. (Ed.) C. West Churchman and P. Ratoosh.
John Wiley, New York, 1959. 64s.

THIS book comprises a collection of papers read at a symposium on Measurement held during the December 1956 meeting of the American Association for the Advancement of Science. It is divided into four parts: some meanings of measurement; some theories of measurement; some problems the physical sciences; and some problems in the social sciences. The lity of the individual essays differs very widely.

or non-specialist readers of this journal the first part of the book has the interest. Although a number of hares are duly chased, there seems coherent unity of purpose which is not apparent in the last section. ing the failure of a Committee of the British Association before me to any definite conclusions about how to define measure- her 'quantitative estimates of sensory events' are meaningful, up has attempted to tackle the same problems.

ho contributes the best chapter in the book, allows a easurement as 'the assignment of numerals to objects ule—any rule'. Some other authors would exclude inal' measurements, e.g. the arbitrary numbering , which implies no meaningful order. Stevens significance attached to interval) with two other ithmic interval scales with arbitrary zeros (e.g. ratio scales (e.g. Kelvin temperatures, lengths,

etc.). The danger lies in applying unjustifiable statistics to the less precise scales. As a trivial example (not in the book) a manufacturer makes some product by five different processes and a panel of experts grades the quality of the products on an ordinal scale: $1 < 2 < 3 < 4 < 5$. In making some calculation of economic significance, the manufacturer then assumes that the improvement effected by process 5 over process 4 is equal to that of 2 over 1 (after all, $5 - 4 = 2 - 1$ is perfectly sound arithmetic); or worse, that 4 effects twice as much improvement on 1 as does 2, since $4 \div 1 = 2\ (2 \div 1)$. Hidden fallacies of this kind are not uncommon in industrial practice.

It is naturally tempting for those practising less exact sciences such as sociology, economics, and some branches of psychology to introduce mathematics wherever possible. After all, powerful sciences like aeronautics, nuclear physics, and chemistry are impregnated with mathematical formulae. Similar formulae would at least make economics more impressive reading and perhaps more powerful. As Professor Knight is quoted as saying: 'If they can't measure, they measure anyhow'. Stevens's article is an admirable deterrent to this kind of activity and comes well from a man who has himself dared to defy Bergson's ban on psychophysics. Is it absurd to speak of measuring unrepeatable sensory events? Well, at any rate we can record verbal and other responses to stimuli and, if these prove to be consistent within and between subjects, who shall forbid us to class them as measurements? Sensory events are changed by measuring them: so are certain properties of electrons and John L. McKnight reminds us of Margenau's ideas on the meaning of complementarity. 'There is a unique theoretical state function for each electron which may be used to give any of its observable properties. . . . One must operate on this state function with some mathematical operator functions to find the expectation values of the classic variables of state.' A very close parallel would seem here to exist w the measurement of sensory events.

Stevens also cites experimental evidence to show that for all 'pro continua, which include the quantitative aspect of things, Plateau' law fits the facts much better than does Fechrer's semi-log relati is suited only to 'metathetic' (qualitative) continua. This c interesting way with certain trends in physics recently di journal.[1] Space does not permit the reviewing of an in of operationism and a totally inadequate condemnati perception by other writers.

Some of the last chapters of the book may perha specialists in the jargon in which they are written ar matising': but Stevens's article alone, not to men on physics, fully justify a warm recommendatio

[1] This *Journal*, 1961, 11

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THE USE OF BEHAVIOURAL LANGUAGE TO REFER TO MECHANICAL PROCESSES*

D. M. MACKAY

1. *A Mis-stated Problem*

'BRAIN-LIKE MECHANISMS' are nowadays something more than a possibility in principle. On all hands we hear of 'programmes' for behaviour which, if only crudely approximate to the human, seems undeniably to occupy some of the right dimensions. Philosophical enquiry into the powers and limitations of such automata is frequently confused by a failure (even among philosophers) to recognise the elliptical nature of much popular talk about computers and their ilk. When the enquiry takes the form of a comparison between the artificial and the natural, it becomes particularly important to sort out the concepts and relationships on each side, so as to see which correspond to which—and to discover which, if any, have as yet no counterpart. The 'semantic chart' of Table I is an attempt to do this for some of the key notions. 'Natural' concepts on the left are matched by 'artificial' counterparts on the right.[1]

[illegible]ssembly of components occurs by 'growth' and 'construc-[illegible]ctively. If the end-product of construction (which might, [illegible]lude artificial growth-processes) be termed a 'machine' [illegible]', then what invites comparison or contrast with it is the [illegible]tural growth—strictly (and only) the 'brain-and-body'

[illegible]ual Conference of the British Society for the Philosophy of [illegible]ptember 1961. In revising this paper for publication I have [illegible]structive criticisms, particularly those of Professor Dingle, [illegible]r David Rioch.

[illegible] only the two 'mechanical' (or the two 'personal') [illegible]e another. Personal activity and mechanical activity [illegible]s but correlates.

in its physical aspects. If the machine be termed (in Sir Geoffrey Jefferson's happy phrase) 'a mass of wires and valves', the natural counterpart would perhaps be 'a mass of cells and things'.

After this point, however, our two columns begin to show some similarities. The (mass of) wires and valves *carry signals*. So do the (mass of) cells and things. Each is organised to form what we term

TABLE I

NATURAL		ARTIFICIAL	
Personal aspect	Mechanical aspect	Mechanical aspect	Personal aspect
Person (Joe)	*Growth* *Brain-and-Body* ('Mass of *cells* & *things*') Carrying *signals* Forming an *information-system* Organising observable *behaviour* *indicative* of	*Construction* *Automaton* ('Mass of *wires* & *valves*') Carrying *signals* Forming an *information-system* Organising observable *behaviour* *indicative* of	? ?
Thinks, feels, hopes, fears . . .	Thinking, feeling, hoping, fearing . . .	Thinking, feeling, hoping, fearing . . .	?

an 'information system'—a system whose activity depends o
reception, transformation, and transmission of informatio
produces observable behaviour (both internal and external).

On the 'natural' side, we have a second column, w
labelled 'Personal aspect'. In this aspect, we are
recognise the being before us as our friend Joe Smith
meet later). We say that Joe is capable of such act
feeling, hoping, fearing. Of these activities w
knowledge only in our own case; but we
indications' of the same in Joe's behaviour.

The problem I want to discuss is repre
marks in the corresponding column on the ri
expressed by asking 'Can a machine think

raged between those designers of automata who regard this as an empirical question answerable by experiment in the affirmative, and those philosophers who share the same presupposition but regard the evidence to date as inadequate.[1]

I have long argued[2] that empirical ' predictions of impotence ' (' you'll never get a machine to do *x* ')—or counter-predictions—cannot resolve an issue of this sort, for any detailed test-specification for particular behaviour, whether statistical or determinate, is implicitly equivalent to a description of at least one mechanism to meet it. Attempts to get round this and prove that ' minds are not machines ' by invoking Gödel's theorem (as Mr J. R. Lucas[3] does, for example) are I believe both hopeless and needless.

They are hopeless because only an uninteresting type of ' machine ' suffers significantly as a result of the impotence shown to follow from Gödel's theorem. Lucas argues : ' . . . Just because the mind has the last word, it can always pick a hole in any formal system presented to it as a model of its workings ' (whereas the type of machine that he considers cannot). Yes indeed ; but the only class of artificial mechanism worth considering as a candidate for ' mentality ' is of the self-adaptive or self-organising type, from which exactly the same behaviour could be expected as Lucas claims for ' the mind '. (I hope to elaborate this point elsewhere.)

But such attempts to save the ' argument from impotence ' seem lso to be needless ; for if our ' semantic chart ' is correct, the short swer to a claim on empirical grounds that ' machines can think ', hat ' minds are machines ' is not ' you are empirically justified (or stified) ' but ' you are talking nonsense '. For in the human case, *brains* that are said to think, feel, hope, fear, but *people*. To say ss of nerve-cells ' thinks ' or ' fears ' would not be an empirical ut a misuse of language; and just the same is true for a mass l valves.[4] If a ' mass of wires and valves ' invites com- human being at all, it can only be with the ' mass of

ing, ' Computing Machinery and Intelligence ', *Mind*, 1950, Lucas, ' Minds, Machines and Gödel ', *Philosophy*, 1961, **36**,

dlike Behaviour in Artefacts ', this *Journal*, 1951, **2**, 105-21 ; The Epistemological Problem for Automata ', *Automata* Press, 1955, pp. 235-51

chanism to Mind ', *Trans. Vict. Inst.*, 1953, **85**, 17-32,

cells, etc.' that comprise his *brain and body*. Thus no degree of resemblance to the performance of the human brain, however great, could justify the semantic absurdity of saying that any ' mass of wires and valves ' can think.

Yes. But is this really a sufficient answer to our restive designer of automata ? May he not justly retort : ' All right : it was silly to argue whether " mechanisms " could think. But insofar as we agree that human thinking *is* bound up somehow with natural brain-activity, is it not meaningful to ask whether, and on what conditions, some kind of " thinking " might not be similarly bound up with artificial brain-activity ? ' He is now asking a very different question, and I think it is a good one. It leads at once to the more basic question of the *nature of the transition* which we are accustomed to make on the ' natural' side, from talking in bodily terms to talking in personal terms—from the ' mass of cells and things ' in column 2 to ' Joe ' in column 1. What are the preconditions of this change ? Under what circumstances is it justified ? Is it purely optional, and if not, in what sense ?

In an attempt to explore this issue one uncovers a trail of prior questions which leads surprisingly deep into the jungle of our basic physical concepts ; for on examination the problem turns out to have some features in common with several others that have plagued philosophers of science. But it is only part of this trail that I propose to follow in the present paper.

2. *The Necessity of ' Underspecification '*

Let us begin with the fundamental notion of goal-pursuit, whi
a basic feature of living systems, and of all artificial devices wit
pretensions to human powers. A self-regulating mechanis
as a thermostat, is said to show ' goal-directed activity '.
repertoire or range of ' possible ' activity, it is said to make
by adjusting a ' control ' or ' switch ' on the basis of ' in
to the disparity or ' mismatch ' between the current st
what is termed the ' goal-state '. The activity selec
to reduce the disparity.[1]

The whole description bristles with diffic
mean, first of all, by ' possible ' here ? Sure
might argue, the only possible activity is the a
what sense then is the activity ' controlled '?

[1] See above, p. 91, n. 2, first re

switch been in a different position, the flow of heat would have been different'—but it was not, and could not be, for good, well-determined, physical causes. So in what sense was any other activity 'possible'?

The answer seems plain. Only if we *under-specify* the situation, by using generic terms that allow more than one microstate to be compatible with our description, can we give meaning here to the word 'possible'. 'A 2-way switch is a device with two possible states . . .' Yes. But *this* '2-way switch' is a physical array of molecules in the *only* state compatible with its past history. 'Is no other state possible?' —Not in these terms. 'In future', of course, it is 'possible' that it may be found in either of the two states—but only if we have not specified the time, nor the intervening history of forces acting on it. Given these, the concept of a 'range of possibilities' evaporates. It is not the physical situation that possesses a range of possible states, but our specification of it—if sufficiently open.

A similar point can be made regarding most of the concepts of applied physics, if viewed from the standpoint of classical molecular theory. Consider for example how the concept of 'entropy' appears in statistical mechanics. It is often asked how an 'irreversible' process (increase of entropy) can arise within a theory based on the 'reversible' laws of Newtonian dynamics. The answer is that (statistical) entropy is definable within the theory only when the system is underspecified in such terms that it can be said to have a range of 'possible' micro-states. Entropy here measures a property, not the molecular situation, but of the summary abstraction (in roscopic terms such as pressure, volume, temperature) which is lly our only available state-description of it. Given a full micro-on in molecular terms, all but one of the probabilities p_i in the efficient, $\Sigma p_i \log p_i$, become zero, and entropy vanishes both and conceptually from the situation as described.

eversible here is the decline in 'specifying power' of a se terms offer too few degrees of freedom. In its e Second Law of Thermodynamics tells us that es change in such a way that anything less than a becomes progressively less adequate as time uences of 'losing track' cannot be undone

together too remote from the 'objective' 'physical' $\Delta Q/T$, it should be remembered that the power to

What then of 'information'? Upon what conditions is it meaningful to speak of 'information-flow', or to say that an event B *betokens* or *conveys information as to* the occurrence of another, A? Clearly, once again, only when we can say that B 'might have been otherwise'. If not, then B conveys no information, in either the common or the technical sense.

But we have just seen that the meaningfulness of such talk depends entirely on the degree of underspecification of the situation. Basically, it is to *us* that B conveys information about A—if we are sufficiently ignorant. To say that 'information is conveyed to the switch' would be meaningless, if by 'the switch' we meant 'that particular molecular array of brass and porcelain'. To talk the language of information-flow is to deflect discussion from the physical situation, to our underspecified[1] state-description or model of it in which 'the switch' means a 'skeletonised' functional variable with two possible values. In this state-description, it makes good sense to say that B determines the state of the switch according to the nature of A, or that information about A determines the selection of activity by the switch.

It follows that to ask whether the activity of a given mechanism is an example of goal-directed or other 'behavioural' activity is to ask, not about the physical situation *per se*, but about the types of underspecified state-description that it can justify. While not every situation can be made to manifest goal-directed features by suitable underspecification, it is quite possible for one level or mode of specificatio to bring out such features while at another level they become total meaningless.

3. *The 'Underspecified' nature of human activity*

So far we have considered only artificial automata; b not recognise the same principle as applicable to hu Insofar as a standpoint is possible from which most, if activity can be viewed as physically determinate, t manifestations of 'goal-directedness' and the like d

extract work from such a molecular assemblage depend which its energy-configuration can be ascertained. Th seem a highly subjective concept turns out to be of cru of engines.

[1] Underspecified, not in the sense of leaving any the sense of being compatible with more than one s

perception on one's choice of level of specification. Here, however, I want to give the argument a new twist, in terms of an important and singular fact about the human brain-mechanism. The intriguing feature of each human being is that, for himself, his bodily state is always and *necessarily* unspecifiable in full detail, so the corresponding 'range of possibilities' of which he is aware is logically irreducible.[1]

Even if his brain were (to an outside observer) a fully-determined physical mechanism, it could never embody an up-to-date and complete description of its own total state. The most fully-supported predictions of the most completely-informed observer (granted full 'classical' physical determinateness) can be for the agent logically-indeterminate, until after the decisions they purport to predict have been made.[2] It follows that no mechanical account of an agent's bodily activity, however deterministic, can logically undermine the meaningfulness, for the agent, of his personal account.

The meaningfulness of our recognition of 'mental' qualities, such as goal-directedness, in the activity of one another is, by the same token, dependent on our being 'underspecified situations' to one another. At first sight this may seem an oddly precarious and arbitrary condition. From a 'Newtonian' view of bodily mechanism it would surely follow that by totally contracting out of a community of discourse (if this were possible), an individual could in principle adopt a fully-specified state-description of the bodies of his fellows, in which concepts of personal agency could rationally find no place? I would ot personally deny this. No demonstration in terms of observable haviour could, I think, rationally *compel* such an individual to accord onal status to the activity of his fellows. In that sense, recogni- f persons is 'optional'.

t I would emphasise, on the other hand, is that insofar as two eract in 'dialogue', and thus become one system, the crucial *necessarily* fulfilled, since for each individual his own derspecified) situation becomes a determinant of that of which thus shares in the underspecifiability-to-him.[3]

rt from the complications due to Heisenberg Indeterminacy, resent purpose.

he Logical Indeterminacy of a Free Choice', *Mind*, 1960,

ved as a physical organism, we may picture a unique r 'information-flow-map' that would show how behaviour is organised. In goal-pursuit, some of the

We seem then to be approaching the view that the requirements for the recognition of personality in natural human beings are of two kinds. First, and obviously, (*a*) the physical 'bodily situation' must admit of and support in practice an underspecified state-description showing appropriate behavioural features: the 'observable indications' of thinking, feeling, hoping, fearing, etc. But the second, and no less important, requirement is (*b*) that we, the arbiters, must be rationally able and willing to adopt the appropriate standpoint of interactive relationship, from which the situation acquires some of our own necessary unspecifiableness-to-ourselves. This I think is the technical correlate of the much-discussed distinction between the 'I-Thou' relationship and that of 'I-Object'. In the latter, no comparable barrier of principle prevents full specification of the object-situation by the observer, even where the object is a normally functioning human body. By withholding the commitment implicit in dialogue, or even in less-discursive forms of interaction—the opening of one's controls to the action of the other—the observer could in principle obviate the systematic mutual 'underspecifiableness' that logically undergirds the structure of personal language.

4. *The 'Personality' of Automata*

What then of artificial automata? There is room for serious doubt in principle[1] whether our understanding of 'what it is to be a man' could ever be made explicit enough to enable us to write a full specification for human behaviour. To that extent, our prospects of fully meeting the first of the above requirements (*a*) are rather unsatisfactory. How should we ever know that we had left out nothing essential?

But to take refuge behind such barriers (real though I believe they are) would be to evade the issue; for there seems every reason to believe that the main lineaments of intelligent human be

lines of flow of information extend outwards to the goal and system.

In the relationship of person-with-person, the significant tion-flow-maps of the two individuals merge inextricably is allowed to affect the 'goal-settings' of the other (as o lines on the map form cross-links that unite the two int of mechanistic analysis.

[1] D. M. MacKay, 'Mentality in Machines' (thi *Aristot. Soc. Suppt.*, 1952, **26**, 61–86, esp. 86

in principle be reproduced in automata, even employing only the techniques known today. We may not feel confident that the 'personality' in question would be fully human, but we can hardly doubt that as far as behaviour is concerned, at least the minimal criteria for such activities as 'thinking', 'learning' and the like could be met.

The real obstacle, then, appears to lie in our second requirement (*b*). Those who wish to refuse personal status to artificial automata are on strong ground ; for we have seen that at the cost of sufficient 'detachment' even the personal significance of human activity (at least of other people's!) can be made to disappear, from a standpoint which views it as fully-specifiable in principle. No observable performance can logically force anyone from such a position.

What would be logically possible (though somewhat perverse) as an attitude to human brain-mechanisms other than one's own, may thus seem more than justifiable towards artificial mechanisms that only feebly imitate them ; and I would not wish to disturb anyone from a defensive agnosticism here. In relation to existing automata I confess to sharing it, for I cannot see myself rationally sustaining a long personal relationship with any I know of.

But we are met as philosophers rather than as psychologists ; and I think the matter cannot be left thus. We must ask, not whether such a commitment would be undesirable, but whether, and in what circumstances, it would be *absurd*.

Let us first take the stock example of the man who adopts a personal
itude to his car, his golf ball, or some similarly passive victim of his
In cold blood, of course he admits this to be absurd. But the
anguage of the garage or the golf-course betrays another level of
ment, which at the time may feel curiously justified. Why?
think we need go farther than the principle we have already
To the extent of its *involvement* in his struggles, his image
the ball, becomes irreducibly *underspecified* through
the rest of his own activity. Insofar as its activity is
reflection of his own, to revile it in personal terms is
surd, however medically, morally, and spiritually
his adjurations were to continue later, in the
od, could we bring home a charge of total
is now fully specifiable to him, and its tempo-
endent on the underspecifiability acquired
anished.

Consider now a so-called ' chess-playing mechanism '. Clearly one can approach such a thing as a fully-specifiable ' mass of wires and valves '—a physical situation that simply unfolds inevitably, and admits of no description in personal terms, of a game-playing or any other kind. But the decision to *play a game* against ' it ' reflects a new kind of commitment, and brings out a curious terminological difficulty. In playing a game against a human opponent, we do not say that we are playing against his body, or even his brain, but against *him*. With our ' artificial opponent ', however, we have no conventional term to allow of the corresponding distinction. Since the term ' mechanism ', as we have seen, is strictly on a par with brain-and-body, it would be a solecism (though a common one) to say that we play against ' the mechanism ', just as it was a solecism to speak of a ' chess-playing mechanism ' in the first place. (Compare ' a chess-playing brain ' *vice* ' a chess-player '.) Perhaps ' artificial opponent ' will serve as well as any term to keep our thinking straight. It is against ' him ', rather than ' his ' mechanism, that it is proper to say we have decided to play. By our practice, we have in effect committed ourselves to an entry in the ' artificial personal ' column of our semantic chart.

But have we not here begged the question by an insidious linguistic manoeuvre? ' Surely ', it may be objected, ' what we want to *discover* is whether a chess-playing mechanism has a personality, or is nothing but a mass of wires and valves ? How then can we begin by speaking of " him "?' I do not think we have jumped a step. What we have been doing is to bring out the fact that the decision on th issue of personality was here implicit in the decision to *play a gam* as distinct from merely poking at a (fully-specified) mechanism. speak of ' playing a game against ' X is already to personalise is always open to us, as I have insisted, to refuse such personal ment : but once we are prepared to make it, the operative seems to be whether and for how long the developing situa us rationally to sustain it.

5. *A Question of ' Consciousness '*

But is it the only question? Suppose, for ex in a chess game by correspondence. Five p visaged and contrasted with profit :

(i) Answering moves selected by a ra predetermined list.

In this case we would soon come to doubt that we had an intelligent opponent.

(ii) Answering moves taken from the record of (one side of) a human chess match.

Here again pointlessness and illegitimacy would soon lead to detection.

(iii) Answering moves selected by a random process from among those legitimate at each stage.

In this case detection would depend on the pointlessness of the moves—the absence, except sporadically, of connections and tendencies interpretable as *conative* and *responsive*. Once the suspicion dawned, we could rapidly (with courage) test it by exposing ourselves to obvious attacks, or making obvious threats.

(iv) Answering moves selected by a well-informed, goal-directed, anticipatory computing process (with a suitable degree of disciplined spontaneity[1]) from among those legitimate at each stage.

Here (as Turing argued some years ago[2]) there would seem to be no behavioural reason for us to doubt the ' mentality ' of our opponent. We might well be beaten with reasonable frequency. Moreover, an artificial opponent of the right (' adaptive ') sort could readily show all the signs of ' learning ' from ' his ' growing experience of us and our ways.

(v) Answering moves selected by a competent human opponent.

What has (v) added to (iv)? In respect of ability to play chess *ex hypothesi*), nothing. The crucial point is surely that in case (v) re is at the far end *someone who knows he is playing :* someone who d, if asked, talk *about* the language of chess, as well as in it—and other things too.

tempting to compare our artificial opponent (iv) with an rm of ' idiot savant '—prodigiously able to play a calculating void of the capacity for normal dialogue. This, however, beg too many questions, for nobody doubts that an *cious* ; but consciousness, whatever we mean by it, is grant on the score of mere chess-playing ability. e, after all, (however improbable) that under some

the Combination of Digital and Analogue Computing nalytical Engines ', 20 May, 1949 (reprinted as Appendix tellect ', *Proc. N.P.L. Conference on ' Mechanization of* London, 1959, pp. 37-52)

kind of selective anaesthesia or surgery a human being might retain the ability to play coherent chess, as it were mechanically or ' in his sleep ', though quite unconscious in the normal sense of ' unaware of what is going on '.) Our criterion of ' consciousness ' would not be in terms of his power to react appropriately to chess-moves, but rather his power to raise his eyes from the board, as it were, and engage in dialogue with us (or at least to think) *about* what he was doing.

The fact that this power could sometimes be difficult to test or demonstrate in practice (as it notoriously is in certain forms of schizophrenia) should not blind us to the reality of the distinction. After all, if thought is in any way reflected in brain-processes, there would presumably be some internal operational difference, that could in principle be exhibited, to correspond with that between consciousness and unconsciousness, even if the subject were resolutely dumb and unresponsive.

In evaluating claims for artificial mechanisms, then, it seems important to distinguish the question of ' personality ' from that of ' consciousness '. Our artificial opponent (iv) has a ' personality ' (of a thinnish sort) which all who are prepared to make the necessary commitment (by playing against him) can come to know. But he would be a bold (or an easily-satisfied) man who would describe that personality as ' conscious ', in any normal sense.

6. *The Logic of Personal Recognition*

It is not my present purpose to discuss ways of remedying
behavioural deficiencies we have noted in the artificial opponent
I have done so on other occasions [1], and many others are active
field. I think it is fair to say that no barrier of principle prev
artificial opponent from showing as many characteristic huma
as we care to specify, including the crucial ability to develop
an internal running representation of 'What is going o
able to converse intelligently and purposefully *about*
and experience. If we are not to make a behaviour
of consciousness, then we will do no harm, and wil
considering a hypothetical case in which *all* such
been satisfied by the activity of the artefact in
What I have been trying to bring out is th
of deduction and commitment—that seem

[1] See p. 91, notes 2 and 4, p. 96, n.

transition from physical to personal ways of characterising such activity, whether in artificial or natural organisms.

Let us take the natural case first. Here is a mass of brain-cells, a going concern, ' wired ' to the organs of a human body. The body (including its brain-cells), we are told, is Joe Smith's. We find that in addition to studying the body as an object, we can also talk to Joe, and he can answer us intelligently, giving all the usual human signs of ' having a mind of his own ' and ' knowing what he is about '. But a doubt strikes us. What proof have we that Joe is conscious? What proof that there is a ' Joe ' at all, as distinct from the mass-of-cells-plus-body? Could not all the behaviour we witness be due to ' nothing but ' the ' dead ' mechanics of brain cells and the rest, responding automatically to the stimuli we supply? Could not the same behaviour in fact be expected if the mass of brain-cells were replaced, perhaps piece-by-piece, by an identically-organised *artificial* ' brain '? If so, why speak of ' Joe ' in this case and not in the other? If not in the other, why in this? What kind of evidence could convince us?

If what I have been arguing is correct, then there is *no* kind of evidence that could logically convince us at this point ; for it is not on *evidence* that our missing conviction depends. What we are suffering from is not a failure of evidence, but a failure of nerve. Given that nothing is abnormal in the ' works ' of Joe's brain, so that the ' indications ' are up to standard, then the transition, from observing the activity of brain-cells, etc., to talking with Joe, is up to us. If we are prepared to ' open up ' and allow the ' cross links ' of which we spoke lier, to form between Joe's system and our own, then our experience e new relationship will be the normal human experience of ue with Joe, a personal agent like ourselves, whose consciousness d (not ' proved ') by the character of the dialogue, as our own rs (including Joe).

in this sense the transition is ' up to us ', yet once it has cessfully) it would be misleading to describe the recogni- ttributes as ' optional ', for it has a certain irreversi-

level, Joe will have established some personal on Joe. We shall not be able lightly to tamper ple, nor feel free to dismantle his body. Even not be able to justify ignoring him as a person one of us ', a member of the linguistic com- y virtue of the particular *stuff* of which his

brain is built (though we have so far assumed this to be naturally grown) but by virtue of the particular kinds of mutual interaction that it can sustain with our own—interaction which at the personal level we describe as that of person-with-person.

At the technical level, he can be recognised as one of us because (and to the extent that) in dialogue our dispositions in the information-flow-map are symmetrical, our relationships reciprocal. In terms of the pattern of information-flow, our two coupled brains engage in a kind of ' resonance ' (in the technical, not the mystical sense!) ; and as a result, each becomes incapable of representing the other as a fully-specified system.

In this sense at least the common remark, that persons in dialogue present an element of *mystery* to one another, has a solid technical justification. Note, however, that this kind of mystery stands wholly apart from any physical puzzles presented by the workings of the brain ; for it would remain even if all of these were solved. No matter how detailed our understanding of Joe's neuroanatomy, we cannot in principle reduce the Joe we know and address as a person to any exactly-specified assemblage of mechanically-determined molecules. If we try to do so, Joe, as a person, must simply disappear from our conceptual framework, as inevitably as statistical entropy does from that of Newtonian dynamics. To say (as some might) that talk about Joe is merely a ' portmanteau translation ' of talk about Joe's brain-cells, is thus an error. The two are indeed (*ex hypothesi*) correlates ; but they are not translations, for the trouble with the molecular level is precisely that *has no words* (because logically it has no place) for the concepts of personal.

7. *Conclusion*

So much for Joe. For all purposes of description and di
is ' conscious ' when he can join as one of us in bearing w
events of our common world. We do not need to ask
is made of, nor how it works. If parts of it wear out
them replaced by exact electronic or other equiv
reason to change our attitude. If eventually Joe
artificial, but functionally indistinguishable—lik
thetical automata—we should surely still have

Surely? Like good oysters, let us not see
last grain of irritation. For imagine that
discuss Joe's consciousness in Joe's hearing.

and listen. Our judgment on the matter may depend critically on the kind of relationship we can establish. We may or may not succeed in getting round this by internal examination of Joe's brain. We may argue long about the optional nature of the judgment. But—and this is the point—all the time, there would in fact be Joe, listening to us, raising a mental eyebrow, and *knowing* the answer. Nothing optional or conventional about that. Consciousness is a matter of fact, on which we are either right or wrong. In this case, there *is* someone there. Joe knows, for it is he.

Clearly, then, any dogmatic dispatch of this question in relation to artificial 'brains' is out of place. Unlike Turing[1], I do not believe that Christian theology supports any objection to the possibility that persons might be thus artificially 'begotten', which I personally find fascinating. I know of no coherent argument against it, and I have been at pains to show that mechanical explicability at the molecular level could not provide such an argument.

On the other hand, it seems equally clear that we are not at liberty to settle the question as a matter of convention, and say that artificial persons that behave-as-if-conscious simply *are* conscious. When deciding whether to *treat* psychiatric patients, or laboratory animals, or artificial persons, or even one another, as conscious, such conventions may indeed be inevitable ; but they can do nothing to relieve our ontological doubt.

No, the simple truth is that we cannot know. And let no one try e old line that it is therefore *meaningless* to wonder whether there is 'Joe' listening to us as we discuss the behaviour of a body before r that body could well be his own.

ity of Keele

[1] Op. cit.

PHILOSOPHICAL ASPECTS OF COSMOLOGY*

R. HARRÉ

IN discussing the philosophical foundations and logical structure of cosmology we must begin by recognising that cosmology as it is traditionally practised includes three distinct branches or sub-sciences. I shall distinguish these as *cosmogeny*, the study of the origin of the Universe ; *cosmography*, the study of the physical structure of the Universe ; and *cosmophysics*, the study of the processes of the Universe. I intend to argue that not all of these studies are scientific, in what I hope is an acceptable sense of that rather vague characterisation. I distinguish these cosmo-studies from the more restricted studies of astronomy, *astrogeny*, the study of the origins of particular classes of heavenly bodies ; *astrography*, the study of the physical structure of finite regions of space ; and *astrophysics*, the study of the processes in stars, nebulae, galaxies, etc.

By cosmo-sciences then I understand studies which aim at describing and explaining the physical Universe. I shall assume that the methods of the cosmo-sciences are to use as far as possible the concepts and results of the astro-sciences to provide the description and explanation of the physical Universe at which they aim. If this is correct then we are plunged into philosophical problems directly; for it is by no mean clear that we are justified in accepting without question the peculiar cosmological extension of the concepts of physical sciences. P matic success and theoretical elegance could be achieved if cosm as it is practised, turned out on examination to be large-scale as and not genuinely *cosmo*logical at all. The philosophical arise because of the peculiar character of what it is we ar and explaining in cosmology. Are we aiming at really explaining the *Universe*, or are we actually aiming a explaining some bounded or limited part of the Uni age of the earth is clearly an empirical question, b the *Universe* is not clearly an empirical questio galaxies and nebulae retreating from each oth question, but whether or not *space* is exp empirical question. The central problem is

* Given to the Annual Conference of the B of Science, Oxford 22–24 September, 1961.

by the expression ' the Universe '. There seem to me to be the following logical possibilities :

1. By the ' Universe ' we mean ' all that there is ' and it is to this subject understood collectively that cosmologically extended physical concepts apply ; for example by ' The Universe is tending to states of increasing entropy ' we mean ' All that there is is tending to states of increasing entropy '.

2. By the ' Universe ' we mean ' some limited and finite subset of all that there is ' and it is to this finite subset as subject that cosmologically extended physical concepts apply. For example, by ' The Universe had its origin in a primaeval atom ' we mean ' There is a closed set of physical objects now existing which had their origin in a primaeval atom '.

3. By the ' Universe ' we mean ' all that there is ', but we understand this expression, when it is the subject of scientific propositions, distributively ; that is we predicate properties of finite regions of space and finite accumulations of matter, and we suppose these regions and accumulations to be indefinitely extensible.

Clearly a genuine cosmo-science must be employing the expression ' the Universe ' in the first or third senses and not in the second, for if it were employing the expression ' the Universe ' in the second sense then it would be an astro-science and not a cosmo-science. We must consider then, how astro-concepts are related to cosmo-concepts, and perhaps then we can see how cosmo-sciences are related to astro-iences. We shall find that the first sense of ' Universe ' is unacceptable, that, with certain qualifications, we can make do with the third.

stro-sciences are, I suggest, related to cosmo-sciences in the follow-ys :

Type Elevation. Concepts like ' entropy ', ' total energy ', ., which are introduced into physics to describe limited and es and systems, are applied to the unlimited and infinite niverse ; where the Universe is the set of all finite lly, we apply to a class predicates which are first s of that class. For instance a cosmogenetic theory attaching predicates that are first defined for er-novae, to the expression ' the Universe ', e set of all systems like super-novae and other

lisation. We suppose that all parts of the cal to (Perfect Cosmological Principle) or

different in a well-defined way from, the part observable here and now. By ' different in a well-defined way from ' I mean that postulated differences are derived from observed tendencies to difference by locally established laws of nature. For instance cosmographies can be constructed by indefinitely generalising the visible (or radio-detectable) structure of the local astronomy ; or by employing some distribution law to extrapolate observed tendencies to different structure, as for instance has been done by Ryle. Similarly we might construct a cosmophysics along the same lines.

In order for a theory to be *scientific* I shall demand that it satisfies the following criteria :

1. In constructing its propositions there are no violations of the principles of logic.

2. (i) The deductive consequences of a theory must be such that it is logically possible for the results of observations, when expressed in the conceptual system of the theory, to be formally contradictory of them. These I shall call ' conflicting observations '.

(ii) The results of conflicting observations, when expressed in the conceptual system of which the original theory is itself a part, must be such that *either*

(*a*) they are interpretable as restriction upon the range of application of the unmodified theory ; *or*

(*b*) they are capable of being made the deductive consequences of a new theory, related to the original, in such a way that the required modification of the original theoretical concepts is a function of the conflicting observations expressed in terms of the conceptual system of which the original theory was a part. For instance, the conflicting observations of Amagat, Andrewes and others to simple gas laws, can be used either to restrict the range of cases for which simple gas laws are held to hold, or they can be expressed in terms of p size and intramolecular forces, and so used to formulate Van der Wa within the conceptual scheme of the molecular theory of gases. theory is scientific if, should there be conflicting observations, they either to restrict the range of the theory or to correct it.

The theses which I shall argue for are :

I. Cosmogeny is *not* a science, but that *eithe* theory is false ; *or* is purely speculative.

II. Cosmography is a science if its proposit indefinite generalisation of astrographical cosmographical propositions arrived at by ty intelligible.

III. Cosmophysics is *not* a science, if its

by type elevation, since as I hope to show we cannot construct intelligible conservation principles by that logical move. On the other hand if its propositions are arrived at by indefinite generalisation then it is a science but these propositions are *doubly* open to inductivist doubts ; firstly on the ordinary grounds of their generalisation to unexplored regions of space and time, and secondly on the ground of scale, that just as there are macroscopic interactions which are different from microscopic interactions, so there is no ground for supposing that there may not be megascopic interactions which are different from both macroscopic and microscopic interactions.

The difference I want to bring out between theory construction by type elevation and by indefinite generalisation can best be seen by looking into the ways in which we treat infinities. There are two ways of dealing with the infinite. I shall distinguish the two ways as (i) the use of ' referential infinities ' and (ii) the use of ' descriptive infinities '. The distinction I want to make can best be seen in mathematics. We say that the integers are an infinite set. There are two ways we can talk about this set. We can employ the symbol ' $\aleph_0$ ' to refer to the whole set of the integers, and assert truths about the set by predicating appropriate mathematical properties of the set as a whole. To do this we attach predicates to the symbol ' $\aleph_0$ ' which functions as a referring expression whose referent is the whole set of integers. To employ this method of talking about the set of integers is, I shall say, to use a *referential infinity*.

Instead we might talk about the set of integers by stating a first and a rule for generating further terms. What we want to say the set of integers we say about the rule of generation whose tely repeated application generates the set. Predicates em- this way of discussing the set of integers are predicates to generating rules and particular integers, and not predi- ate to whole sets. To employ this method of talking integers is, I shall say, to use a *descriptive infinity*.

icians, post Cantor, all referential infinities are para- s of descriptive infinities. For example Wilder [1] lements, or its cardinal number is $\aleph_0$ we mean ts and the elements of N (the set of integers) correspondence '. And of course one-one ood in terms of a rule of indefinite applica- of one set against an element from the other,

so that for each element there corresponds one and only one element in N, and we adopt some investigatable and defined starting point for each set. Consistently with this treatment mathematicians' statements in which a referential infinity is the ostensible subject must be reducible in principle to statements in which the referential infinity does not occur but is replaced by a descriptive infinity. But to replace a referential infinity by a descriptive infinity in a statement is to change the *reference* of the statement from the set as a whole, to an investigatable initial subset of it, together with the rule which generates the rest of the set. In cosmology, as contrasted with astrophysics and astrography, referential infinities are apparently in common use, in particular the expression 'the Universe'. The only way for cosmologies to escape the fallacy of predicating a property defined distributively to a set understood collectively is for cosmologists to adopt Cantor-like moves so that referential infinities are paraphrasable out in terms of descriptive infinities. We shall see below that this technique, though apparently promising has residual difficulties. Before we can usefully consider that question, however, it is necessary to look a little more closely into the criteria I am proposing for regarding a theory as scientific.

Our criteria above for deciding whether a theory is scientific are too general for practical purposes. The first or logical criterion can be made specific in two dependent requirements.

(i) The logical calculus of which the theory is an interpretation consistent.

(ii) Terms and predicates are not so conjoined that their combi tion in propositions violates the rules of some form of the theo types; or fails to distinguish between distributive and co predication.[1] But to see whether a theory satisfies what o call the empirical criterion is not so easy. Must we wait flicting instance turns up, or until we can design an expe would produce one? Not necessarily, for there is more useful criterion for our purposes. It is th science must incorporate some conservation prin physical science were not to contain some cons *any* deviation of experimental results from pr sistent with the theory, since lack of cons quantities would imply that there are no

[1] I owe the suggestion of this employment of

which deviations would count as *deviations*, and hence force us to modify the concepts of, or limit the scope of, the theory. If nobody cultivated fields and gardens then no plants would be weeds. On this criterion alchemy is not a science, but astrology is—though the former contained many true propositions and all the propositions of the latter are false.

At first sight it would seem obvious that conservation principles could only hold for systems with well-defined boundaries. That is when we talk about the conservation of mass or the conservation of momentum, we mean that in some isolated and finite part of the cosmos such physical quantities are conserved. But we do owe to Helmholtz a suggestion for a sense for the conservation of certain physical quantities in the cosmos. His move is to explicitly adopt a descriptive infinity as the subject of conservation propositions, so that for instance, the conservation of energy in the cosmos is understood as the principle that for any finite part of the cosmos, however large, provided that it is in energetic equilibrium with its surroundings, within that part energy will be conserved. We shall return to this problem in discussing Thesis III, that cosmophysics is not a science.

The method I propose to adopt in discussing cosmo-sciences is as follows :

A. *The Tests for Logical Adequacy*

(i) Does a theory arise by type-elevation or by indefinite generalisation?

(ii) If it is indefinite generalisation then it is either an astro-science or ...itimate cosmo-science and raises no *philosophical* problems though ...ical problem of universal empirical presuppositions remains. ...pe-elevation, then it can escape predication difficulties, only ... such that all referential infinities are paraphrased out in ...scriptive infinities.

...*for Empirical Adequacy*

... theory postulate unobservable events, things or pro-

... then it is an astro-science and raises no philoso- ... does then are these events, things or processes ...ppened or should happen differently then would ... be different? One must bear in mind that to ...pirical testability as ultimately *proving* any ... its logical form is that of

affirming the consequent of a conditional proposition. (It is interesting to note that one of Kepler's most brilliant essays is a defence of this logical form in cosmology.)

Thesis I. To establish the destructive dichotomy, that *either* any cosmogeny is false *or* it is speculative.

Kant's First Antinomy was the first argument I know designed to prove this thesis. However since it depends upon a quite unjustified theory of the convergence of causal conditions with time, it will not do. I propose the following antinomy as proof of the destructive dichotomy.

Consider a hypothetical first event e_0, at which we propose to locate the genesis of the cosmos. Let us call events which are subsumable under currently known or yet to be discovered laws of nature, *events of the kind we know now*. They are characterised by having antecedents and consequents, since they are subsumed under current or yet to be discovered laws of nature. Let us call events which are not subsumable under currently known or yet to be discovered laws of nature, *events not of the kind we know now*. They are characterised by lacking either antecedents, or consequents, or both.

E_0 is either an event of the kind we know now or an event not of the kind we know now.

(*a*) If it is an event of the kind we know now then it had antecedents, and what these antecedents must have been is deducible from our current or yet to be discovered laws of nature, even if only distributively among alternatives of different probabilities. Therefore it could not have been the first event. That is no first event could have been an event of the kind we know now.

(*b*) If it is not an event of the kind we know now, then either it has no antecedents, or it has no consequents or neither. Now no event satisfying the conditions of either of the last two disjuncts could not have been the first event of our universe, for it could not be connected by natural processes to the universe as we now know it. If the first event having no antecedents, then we *cannot know* what it was, for we can have no laws of nature which we can now know which would enable us to retrodict to an event which had no antecedents. So that to choose some event as the first event, would be arbitrary, for the choice is arbitrary.

We conclude therefore,

Either there was not a first event, and all

Or the first event must

by placing it at one geographical pole, with another contrarotating bucket at the other. The important thing is to see what is involved in denying Cusa's Principle; or in denying jointly its modern subsidiaries—the 'Perfect Cosmological Principle' and the Principle of Isotropy.

To see what is involved let us examine briefly an argument that is on the tip of Newton's tongue, though so far as I know he never explicitly uses it. The Principle of Universal Gravitation suggests that for any finite set of bodies, say the solar system, we can find the centre of gravity. Combine this set with additional bodies to form a larger set, and we can find the centre of gravity for this set. If we continue this process until we have exhausted all bodies we find thus the centre of gravity of the Universe. Now, though in an infinite Universe, we cannot in practice succeed in carrying out this programme, there is nevertheless some limit to which this progression of centres tends, and that limit is the true centre of the Universe. But as Cusa pointed out, if we express his argument in Newtonian terms, an infinite set of bodies has no centre of gravity; or to say the same thing, any point you choose can be made a centre of gravity by enlarging the system you are investigating. Einstein's mathematical universes show too that unless we build in some principle of convergence, say some condition on the gravitational potential at infinity, there can be no reason whatever to suppose that there is such a limit as the centre of gravity argument implicitly assumes.

So what emerges is this: only on a finitist assumption, or on a ...ulative laying down of some convergence principle as is assumed ... invention of such devices as the cosmological constant or Le ... Atom, can we anchor a cosmographical scheme to a fixed

VI. To establish that cosmophysics lacks conservation ...n acceptable form; and that indefinite generalisation to ...m terrestrial physics depends upon a new inductive

...dy noticed that statements containing referential ...ence is an infinite class are untestable, while ...escriptive infinities are testable since the initial ...element is testable. What I now propose to ...ulties about setting up conservation principles ... cosmophysics. For instance consider the ... tending to a

state of maximum entropy.' One way of reaching this conclusion is as follows :

(i) The total mass/energy of the universe is conserved.
(ii) Every natural process proceeds in the direction of maximum entropy.

The route by which the above cosmophysical statement could be deduced from (i) and (ii) is :

(iii) Premise (ii) is either a paraphrase of or has as a consequence (depending on which sort of thermodynamics one espouses) the empirical fact that no natural 'process' is completely reversible without a gain of energy from outside itself.
(iv) The Universe has no external source of energy (by hypothesis) since the Universe is all that there is.
(v) Therefore the total process of the evolution of the universe is an irreversible process.
(vi) Therefore the Universe is tending to a state of maximum entropy.

The general form of such an argument is, as Kant would have put it, transcendental; that is, we predicate closed class predicates of open classes. From mathematics we have learned that this form of argument is legitimate only if the referential infinity is paraphrasable out in terms of a descriptive infinity. But (iv) is not a genuine conservation principle, since it is reached, not by a closure of a class, but by what amounts to a play upon the words 'external source'. Further, this equivocation about conservation is repeated in step (v), for that proposition is intelligible only under the condition that the process of evolution of the Universe is a natural process in the sense of natural proces in (ii). But every natural process of the kind to which t Second Law of Thermodynamics applies unequivocally is a bou natural process if the entropy relation between states is to be true So that the condition implies that the universe to which we are r is a bounded process. Hence we can see that conservati infinite quantity, because there is no more than an infinite some physical quantity, is not conservation at all, in the to generate the argument.

In this argument then the apparent referential i replaced by a paraphrase in terms of a descriptiv generating function incorporated in the descri unlimited in its application, as it must be if we *infinity*, then *a fortiori* its application cannot argument the Universe must be limited in so tion principle to hold in the required sense make sense of

mology. But if it is treated as an argument in astrophysics or large scale thermodynamics, and 'Universe' does not stand for the infinite cosmos, but for some limited part of that cosmos, then the argument becomes intelligible and convincing, since conservation of a physical quantity can be given sense within those limits, no matter how far we extend them.

This is a particular case of the paradox of cosmophysics. If a scientific account of the physics of the Universe is attempted then the account is not *cosmological* because to have conservation principles a limitation or boundary condition is required on the referential terms. If it is cosmological then it is not scientific since no boundary conditions can be laid down.

But, one might object, what about cosmologists' finite universes? These are mathematically intelligible, and assign upper limits to such physical quantities as mass. A moment's reflection on the nature of these universes soon shows one that *either*

> (*a*) Their finitude is an expression of theoretical limitation upon observation ; for instance based upon the escape velocity for electro-magnetic radiation ; *or*
>
> (*b*) They do not preclude 'other universes' necessarily, since to limit space by quantity of matter, is to overlook the logical fact that space is defined not only as the actual relations between objects, but must include an infinite network of counterfactual relations which would hold had there been objects to be the terms of such spatial relations.

The second disjunct I take to be the basis of the Hoyle-Bondi-Gold trinc that there is no reason to suppose that the Universe is not ite, and that indeed no reason could be found.

Even when we have overcome all the logical difficulties and can t the construction of an astrophysics there are still untestable s which are necessarily involved in any form of astrophysics. the observations and experiments upon which we base the s which figure in descriptive infinities are, relative even to niverse, small-scale. So that an assumption lies at the descriptive infinities, or of any device which extra- l to the large scale, even stopping short of infinity. that large-scale processes are the result of some ons observed on a small scale. It is an exact mption that the mechanics of very small the mechanics of macroscopic objects, and of that assumption.

nd downs. In antiquity and

the Middle Ages it was widely held to be erroneous. The properties of quintessential matter were held to be different from those of the matter of the four terrestrial elements. It may be that the success of the sixteenth-century extrapolation assumption is temporary; that taken in sufficiently large scale matter may exhibit new interactions, neither subatomic, electrical, gravitational, inertial or of impact.[1]

We are now apprised of a hazard that is built in to the logic of astronomy. If we must construe all referential infinities as disguised descriptive infinities for our 'cosmologies' to be sciences, and every descriptive infinity is open to the hazards of novelty with scale, every astrophysics is open to this hazard. This is a different hazard from the general hazard of induction, for that arises by the extrapolation of laws spatio-temporally, and is the hazard ruled out of consideration by the adoption of some form of the cosmological principle. The adoption of the cosmological principle, even in its perfect form, does not rule out the scale hazard. To rule this out, even if it were advisable, requires an additional cosmological principle of a quite different kind. It is a principle very seldom mentioned by cosmologists, though always tacitly assumed. Such phenomena as the 'expansion of space' are given explanations, when these are attempted, in terms, for instance, of an electrical force, as in the hypothesis that the causal agent in the expansion is a minute difference in the numerical values of the charges on proton and electron (a difference not observed to well within the order of observation required by the hypothesis).[2] Whereas, so far as I can see, the necessity we are under anyway to employ descriptive infinities, leaves us the option of inventing a new interaction, and basing a new descriptive infinity upon it, of which e.g. the observed redshifts are the initial cases.

Explanation in the Astro-sciences. Given the truth of the [illegible] argued above, what logical principles do we properly employ in the astro-sciences? What counts as a satisfactory explanation of astronomical phenomena? To answer these questions we must look at a 'cosmology', and for reasons of familiarity and simplicity that of Newton.

To derive his concept of inertia Newton ha[illegible]

[1] See D. Wilkinson, *The Listener*, 64, for some interesting [illegible] gascopic interactions.

[2] It is interesting to notice that this very explanation [illegible] F. U. P. Aepinus (1724-1802).

that the mass of a body was not affected by its location, chemical properties, or by its undergoing the acceleration of a force. Why this was so Newton was not obliged to say, since it was in terms of this invariance that everything else in his universe was explained. In addition to the invariance of mass Newton *makes* inertial motion and rest fundamental and not requiring explanation. There is no need to *explain* why a body continues in its state of rest or uniform motion—indeed in the Newtonian system it is *logically* impossible to supply an explanation. Change of place, a process defined as uniform within the system, is not open to explanation, only change of rate of change of place. Forces are the only causal agents, therefore where forces do not act, we do not have the material for a *causal* account of what goes on. Hence inertial motion and rest are, for Newton's system, the fundamental and unexplainable concepts. If there is no *reason* why a change should occur (where 'reason' is defined within the system as the action of its allowable causes) then no change occurs. Astro-scientists are not, therefore, logically able to account for everything that happens. Somewhere in a science, and the astro-sciences are no exception, there must be a process for which there is no *reason*. No reason in the sense of no member of the set of *admissible* causes acting. Contrast the Aristotelian system where there is no reason in physics for downward appetition, but in which therefore what we now call intertial motion would have to be explained. By choosing to make the following identifications :

(*a*) Only accelerations are *caused* changes,

(*b*) The only causal agents are forces,

on closes one channel of enquiry by opening others.

eneral we could say that if we observe or imagine n different rocesses occurring only $n-1$ of those types can get an explana- given scientific system. The qualification 'or imagine' is ce on the whole, explanation proceeds from theoretical nd processes to observed entities, events and processes. ill help to provide two important methodological

of Universal Causality : For *every* natural process account can in principle be supplied.

of Sufficient Reason : Every scientific system hy the physical system it describes is thus and

state

The latter principle, as we have seen, is, for any given scientific system, *S*, limited in its application by the fact that *some* process must be chosen as the unexplained, within the conceptual apparatus of that system. But we can still hold, at the same time as we recognise a restriction upon the Principle of Sufficient Reason, that the Principle of Universal Causality is universally valid. That is to say we can always hope for a deeper or wider system, *S'*, in which *S* is explained, and that no end to this process of piling system upon system can be foreseen.

In other words the Principle of Sufficient Reason is applied in science relative to some particular conceptual apparatus, while the Principle of Universal Causality is applied to the growing set of systems, each of which either explains or supersedes the last.

To give this rather abstract distinction body it is necessary to specify rather more closely what I take a conceptual system to be. A *c—s* can best be expressed in three lists.

1. *The list of classes of existing individuals.* For example, for Newton, material atoms.

Question ruled out

Why are there atoms?

2. *The list of classes of irreducible qualities of these individuals.*

For example, for Newton, the primary qualities; inertia, extension, etc.

Question ruled out

Why does a body possess inertia?

3. *The list of relations between these individuals.*

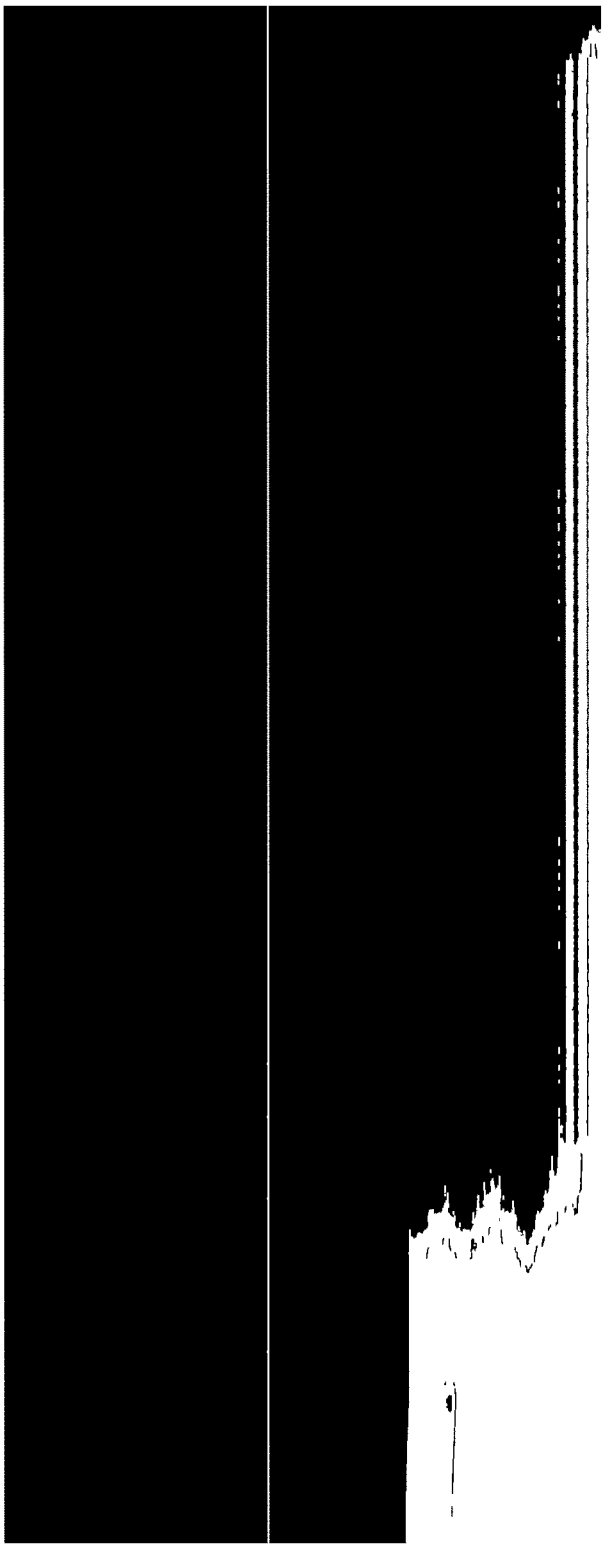
For example, for Newton, spatio-temporal relations, interaction by impact, interaction by gravitational action at a distance.

Question ruled out

Why does one body attract another at a distance?

To supply an explanation of phenomena in terms of the any of these three lists is to satisfy, for the *c—s*, the Principle o Reason. To ask for the cause of gravity, as Newton do places, is to step outside the *c—s* and the necessities of t Sufficient Reason, under the wider necessity of the Prin Causality, for to every *c—s* there is in principle ano it.

The lists of a *c—s* are not compiled straigh are, obviously enough, the results of selectio For example, Kepler selected magnetic int action at a distance in terms of it—Newton s and follows Gilbert in suppo



impact. Furthermore, the *c*—*s* lists *must* be a selection from nature, for otherwise no *explanations* can be derived from them, but only descriptions of independent phenomena. Since there is and must be selection, that selection could always be other than the one actually performed. So that for any given *c*—*s* another can be selected which could stand in an *explanans* relation to the first.

For any given *c*—*s* then the Principle of Sufficient Reason is limited in application to all phenomena, processes, etc. other than those mentioned in the lists—but for any given *c*—*s* there could always be another *c*—*s* in which the items in the lists of the first *c*—*s* could be explained. The Principle of Universal Causality could be understood as expressing the methodological hypothesis that the backing up of one *c*—*s* by another is, in principle, endless.

Every ' cosmology ' is the expression of a particular *c*—*s*, and hence within it the Principle of Sufficient Reason cannot be applied without restriction. If the ' creation ' of a hydrogen atom is a process in Hoyle's *c*—*s* lists, then it is wholly irrational to complain that the process by which this occurs has not been explained. To undertake to explain it would be implicitly to adopt a different *c*—*s*. To require it of Hoyle, Bondi, and Gold is to confuse the Principle of Sufficient Reason as I have undertaken to define it, with the Principle of Universal Causality, that there is *always* logical room for another *c*—*s* to be invented.

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PHILOSOPHICAL ASPECTS OF COSMOLOGY*

W. DAVIDSON

most important philosophical problem in the case of cosmology hether it can be a science at all, and what we are to mean by the erse which it claims to study.

To answer this question we must first consider the facts, the ervational data from the great optical and radio telescopes now in

We view the system of galaxies and clusters of galaxies distri-ed more or less evenly in all directions about us. We observe recession of the galaxies at a speed proportional to distance. We ct radio sources to far greater distances, also distributed about us a remarkable degree of isotropy. In consequence cosmologists pt, with apparent justification scientifically, the well known mological Principle that the universe is spatially uniform at a n cosmic epoch.

We can then assume, until it is contradicted by further evidence, the universe is the result of the linking together of regions of ters of galaxies such as the region we now observe, in such a way preserve a spatial isotropy about every cluster at any given epoch. further suppose that the evolution in time ascribed to our regio lusters is paralleled statistically in every other region. The resu n principle, an unambiguous concept of the universe as a whole. The influence that has been strongest in inspiring confidence concept of the universe, apart from the observations, is the ge ory of relativity. Einstein was the first to show that th ations of general relativity admitted a static, spatially ed model of the universe in a very satisfying way. This universe of course turned out to be inadequate when he galaxies was discovered, but other theoretician an expanding uniform universe could be descri erms of the space-time concepts and field e tivity.

A particular feature of this revelation by ge universe as a whole is not the mere joinin pace such as our own neighbourhood of

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linked in such a way as to form a unity having characteristics in the large which transcend those of the individual regions of space. Thus, for example, it was found that a uniform universe would have a definite spatial curvature, in general a function of the epoch, that could be positive, zero, or negative, and space itself would be correspondingly closed, flat or Euclidean, or hyperbolically open. The field equations of general relativity showed that at any epoch the radius of curvature of space was related to its rate of expansion and to the average world density of matter; the curvature could in fact be determined from these two quantities. Since both the latter quantities are in principle observable, the curvature of the universe as a whole is a perfectly scientific concept.

This idea of the universe as a whole depends, of course, on the validity of the Cosmological Principle. It is very doubtful if any scientific view of the universe in its entirety will ever be available if the universe is not in fact isotropic on the large scale. If, for instance, the universe were hierarchic, as conceived by Charlier, then, because we can observe only the systems of lowest order—stars, galaxies, clusters of galaxies, and possibly superclusters—we should not be able to verify anything postulated about the universe in its totality.

In this connection McCrea has suggested that, quite generally, theoretical distinctions between various cosmological models may be meaningless, because of an essential uncertainty in the observations which increases with distance. Arguing from local physics McCrea asserts that if two bodies A and B act on one another with an effect propagated at a finite speed, then the mere knowledge of the positions velocities of A and B at time t will not permit us to predict what y, will do next. For this depends on details of B at the time he effect left B, and indeed only by studying the behaviour of over a finite time to discover the laws of motion can we reach of being able to *predict* the behaviour of A or B. In the iverse, McCrea argues, the system of bodies that we have probably infinite and therefore, even if we knew the we cannot by observing the universe for any finite ient information to make exact predictions. Therehe laws precisely so that exact laws for the whole ing.

his view undoubtedly underlines real difficulties me. However, in my opinion, it is by ent to justify abandoning the present

aims of cosmology, before they have been adequately tested. McCrea's viewpoint suggests that we ought to forego any attempt at an exact or determinate cosmological theory. This would presumably include the rejection of the Cosmological Principle based as it is on an extrapolation of our admittedly finite observations. But in the light of these observations can anything more compelling and scientifically valuable be suggested to take its place?

If a simple concept of the universe as a whole, such as that provided by the Cosmological Principle, is not to be set against even the present limited optical and radio observations, then what scientific value can be attached to these observations? For instance, even assuming the Cosmological Principle we have the present situation when Ryle has claimed that the observed distribution of radio sources proves that the universe is evolutionary, or at any rate not in a steady state, while Hoyle claims that these observations can be interpreted as consistent with a steady-state universe. This situation, disconcerting as it is, is not too serious because there seems every likelihood that these two kinds of model are already sufficiently well defined that ultimately an irrefutable chain of argument will allow the observations to distinguish between them. But if on *prima facie* examination the same observations can lead to radically different interpretations with the aid of the restricting factor of the Cosmological Principle, then it seems certain that there can be little scientific profit from observations on this scale that are not fitted to any kind of testable cosmic theory at all. For let us make no mistake, if uncertainty in the formulate cosmic laws is proportional to distance, then the universe as a whole a concept eschewed from discussion.

In my view the possibility observed by McCrea that the uni may be spatially infinite—which is not at all a foregone co —does not necessarily make a cosmology more difficult to For, as already remarked, if the Cosmological Principle is p part of the theory then at a given epoch all regions of sp equivalent on a suitable scale. According to present id this case only two possibilities in model type—the fam ary models and the unique steady-state model. steady-state model we have the extra simplifyi all parts of space are always the same as our ow fore, in principle, by relatively local observat all the laws of a steady-state universe, an satisfied further afield.

In the evolutionary type of model it is just the process of looking progressively further into space that allows us to prove that it is evolutionary, and to examine the evolution of the bodies in it. In principle we can follow their history right back to the stages of very high density inferred for this kind of universe, and which distinguish it from the steady-state model. The further we observe the more information we have for formulating a cosmic theory. Thus, the fact that the velocity of light is finite is actually of cardinal importance in reducing uncertainty in cosmology. If we made use only of the 'now' then the remote past and future of the universe would become vastly more obscure.

Another point to be observed, if we accept the Cosmological Principle, is that whatever we witness in the very distant parts of the universe in the evolutionary case must have been realised in our own region of space at the same cosmic epoch. It follows that, whatever we infer about conditions in the remote past from the progressive trends observed as we look further into space, this must be consistent with whatever we are able to deduce about our own past here—that is the past of local clusters of galaxies, of our own galaxy, and possibly even of the solar system.

This argument may be applied in both directions. We may be able to avoid speculation in local sciences, such as geophysics or astrophysics, only if we have a successful scientific cosmology. Thus, if for example we can establish by distant observations that the universe did originate from a state of high density T years ago, then it appears highly probable that a successful theory of the formation of our galaxy, stars, and perhaps even the earth will depend on this fact. Again, universe is in a steady state, such theories of the origin of the stars, and earth must depend only on conditions such as we ow. It seems palpably clear that if in fact cosmic conditions nt to the formation of these objects then, until we discover onditions were, all formation theories must be purely us, it may not be possible to separate the terrestrial and considerations of the large scale character of the uni- progress in these sciences, and the unity of science as on the development of a satisfactory cosmology. that a scientific cosmology, as exact and deter- be essential for science generally, and that in principle if the Cosmological Principle

Fortunately, the Cosmological Principle appears to be very well founded. The recent counts of radio sources, in particular, have revealed a remarkable degree of isotropy out to distances approaching 10^{10} light years, which is of the order of the observational radius of the expanding universe or of the radius of curvature of space. In addition, the special investigation by Leslie to detect clustering in radio sources has shown that there is negligible clustering of the sources out to distances of at least 5×10^9 light years. No significant departure from a random distribution was observed. We can understand this result if the clusters of galaxies, in which radio sources are most likely to occur, are distributed uniformly in space and if there are no more than one, or occasionally two, radio sources in any cluster at a given epoch.

Turning to the evidence of the expansion of space itself, Hubble's redshift-distance relation has now been verified out to distances of the order of 4×10^9 light years. This again suggests the unity of the universe. For it cannot be supposed that this is only a local expansion—that the observed system of galaxies is expanding *in* space and not space itself that expands. For if other discrete regions of clusters even further afield existed without expansion, or possessing their own local expansion (or contraction) *in* space, then we should be faced with the problem of why it is dark at night (Olbers's Paradox). Thus to resolve Olbers's Paradox space must expand, just as we observe it to do, and the isotropy of the expansion on the scale of the cluster suggests that the universe is uniform on this scale—that is, there are no larger systems than the system of clusters.

The very fact that the clusters do recede from one another sugg that any supercluster of clusters would be unstable and disperse b its formation had got very far. There is doubtless a very fund reason why the distribution of matter on a sufficiently l should be uniform. It points to the presence of a deep se force which allows only minor disturbances of unif dimensions are small compared with the radius of cur That is, there is an apparent cosmic stability rather surface tension in a soap bubble, or gravitation on earth. While at present we can only observe th it is to be hoped that one day it will be physical

Further evidence for the Cosmological P concept of the universe as a whole, is pr background radiation in the sky at any

small scale. The situation we have described does not arise in local physics. For in local physics if a gas expands under pressure it loses internal energy, but this energy can be identified as a gain by the system that it expands against. The energy account is therefore regarded as balanced. But in the expanding universe energy is lost from all regions whose boundaries expand with the universe, and no region has a net gain. One can still account in the usual way for the energy lost in any one region, that is as the work done against pressure, but it is futile to look to see where the energy goes—it disappears into the cosmic sink which is directly associated with the expansion of space.

It is perhaps not always realised, even by cosmologists, that the continuous creation of matter suggested by the steady-state school has not really got the revolutionary character so often ascribed to it. All they were doing was substituting a continuous creation of energy for the continuous annihilation inherent in the previous cosmologies. As McCrea was the first to show, this may be achieved in terms of general relativity simply by replacing the usual assumption that the cosmic pressure is positive by the supposition that on the average in intergalactic space it is negative. This hypothesis cannot of course be justified at present, even although for all we know about intergalactic space it might be true. Therefore, provided the observations rule out the steady-state model, and provided they can be fitted by evolutionary models with positive pressure, a positive pressure is the most economic hypothesis to make for the time being.

General relativity shows that, provided the average pressure remains itive, the extrapolation of the present expansion backwards must to a state of very high density occurring less than T years ago, T is the reciprocal of the Hubble constant—about 10^{10} years. orks forward from this point, then the equations reveal that if ure is positive (closed world) the universe will expand to a num extent and then contract; if the curvature is zero or worlds) the universe will expand indefinitely towards a dispersion. The best observational criterion for hese possibilities is the present acceleration of the ex- ral relativity shows must be negative for a positive closed models give rise to a retardation of expan- high, the hyperbolic models to a retardation

does remain positive, and it is certainly else if a state of very high

density is reached, then we have to take seriously the possibility of a cosmogony in the evolutionary models. For according to general relativity there would in this case be no possibility of regarding the passage through a state of very high density as the continuous reversal of a previous contraction. If the universe is of the type that expands to infinite dispersion during an infinite time, there is in any case scarcely a logical reason to postulate a previous contraction.

It is of course hard from our physical experience under present conditions to accept a creation event without antecedents, but it is not necessarily inconsistent with the present observable laws. We have to suppose that at $t = 0$ all the initial conditions were set for the subsequent physical evolution of the universe to the stage at which we now observe it. The analysis of the universe for $t < 0$ may have to be regarded as not within our conceptual system of physical laws. In this connection we have to recall that present quantum mechanics regards the change of energy state when an electron jumps from one orbit to another as a discontinuous process which is not *per se* capable of further analysis. Therefore, the indications of a cosmogony in the evolutionary models is, at this stage, hardly a sufficient reason to reject these models.

If the evolutionary models of positive pressure have to evolve from a singular creative event in the past, then the models of positive curvature, in particular, must be regarded as expanding from the singular state to a maximum size and then returning to an annihilation event in a finite time. The question arises whether this is actually a reversible process in the thermodynamic sense.

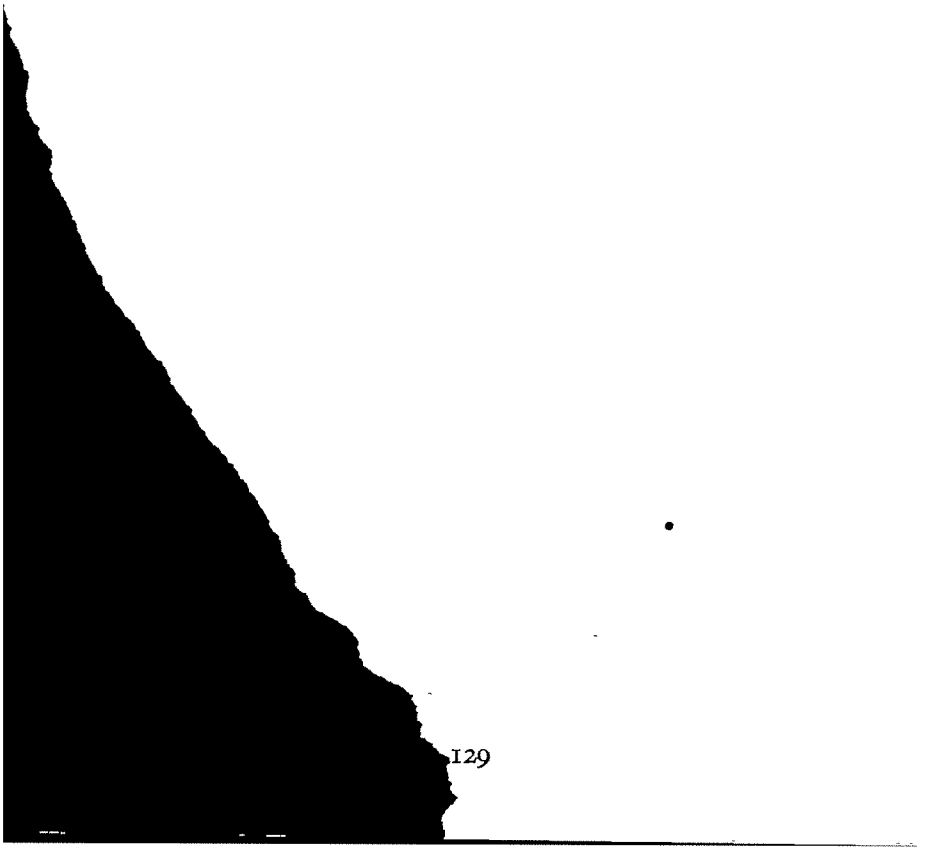
If we consider the regions of clusters whose boundaries expand and contract with the universe, we note that because of the homogeneity of space there is no heat exchange between such regions. Hence the criterion for reversibility in such a region will be whether processes involving friction are present within the region which would produce extra thermal energy, and corresponding increase in entropy, irrespective of whether the region was expanding or contracting.

In so far as there are plenty of sources of friction [illegible] obvious at the present epoch, such as interacting gravitational fields, differential rotation in stars, and since matter is evidently [illegible] with radiation, we have to conclude that the entropy of the universe is steadily increasing at the present epoch. Whether the entropy of the universe as a whole is scientifically [illegible] of that within the regions of

verse is increasing and therefore any ultimate contraction could not be identical with the reversal of the present expansion.

Although the cosmological problem is evidently beset with great difficulties, I hope I have shown that it is not a matter for mere speculation. The present observations indicate strongly that, despite its unique character, the universe as a whole is susceptible to investigation in a scientific manner. Furthermore, in the last analysis, the properties of the universe as a whole are likely to be highly relevant to many terrestrial and astral sciences. Indeed, it seems not too much to say that the logical goal of every pure science should be to find its place in a unified cosmology.

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PSYCHOLOGICAL EXPLANATION: THE 'PRIVATE DATA' HYPOTHESIS *

MICHEL TREISMAN

IF a physicist asks a question such as 'Why is this table solid?' he is at the beginning of an intellectual inquiry in which some familiar concepts such as 'solidity' will disappear altogether, and others, such as 'field' or 'force' will undergo a process of redefinition which may leave them with little more than historical links with their common sense uses. Is this true of psychology: do the words which laymen, and philosophers, use to describe their experience and behaviour also require redefinition or replacement if the needs of scientific explanation are to be served?

In order to examine this problem I shall consider two instances of the development of psychological explanations, one shortly and the other at somewhat greater length. For this purpose I shall give 'scientific explanation' the same meaning as in other areas of science. A scientific theory is considered to consist of a set of statements (laws, formulae) containing theoretical concepts, from which by the appli- cation of certain transformation rules statements can be derived whic refer to observations. If such observations have been made t derivation serves to explain them, if not, to predict them. Thus f certain general hypotheses concerning theoretical concepts su molecules the empirical gas laws can be derived, and these c be applied to particular instances, such as the expansion of as it rises. In every case making an observation requires t stage a human observer must see or hear or touch somethi or report his observation in a statement which can development of the theory.

Human behaviour is commonly explained b purposes and motives of actions, and the emotio explanations may also be given for the behavi actions are interpreted as arising from 'pur

* Given to the Annual Conference of the B Science, Oxford, 22-24 September 1961.

'instinct' and other terms which in ordinary usage have wide and imprecise implications. An early approach to developing scientific explanations for behaviour was to elaborate and systematise the application of these terms, and systems of instincts such as that of McDougall, were set up. This system brought together many otherwise unrelated behavioural observations, bore witness to the broad consistencies to be found in behaviour, and provided a flexible and widely applicable descriptive system. Moreover, since terms were used in senses closely related to their common usage, the system seemed readily comprehensible. However as a scientific account it had a number of defects. It made no predictions save those given by any system of generalisations; and since there were no explicit criteria for assigning its proper description to an observation it was difficult to fit new observations, unfamiliar in pattern, into the system. Each instinct was regarded as made up of Cognitive, Affective, and Conative aspects. This was in keeping with traditional ways of viewing behaviour, but it was difficult to relate to what was known of the physiological mechanisms underlying behaviour. These difficulties, and an increased appreciation of the importance of prediction and confirmation as criteria for scientific theory, led to the development of new ways of explaining behaviour.

These were based on the development of criteria for making and recording observations: the recognition of movements or patterns of behaviour, recorded as such, rather than interpreted. Theories were developed in which terms such as 'impulse' or 'excitation' might occur, but with strictly defined implications, so that their logical consequences could be clearly determined. The processes and relations hypothesized could sometimes be represented by mechanical models, circuits working on cybernetic principles. These models led to predictions which allowed them to be tested; and they specified or gave descriptions of components which in principle and sometimes could be identified at the physiological level. For example, the recurrence of hunger has led to hypotheses about controlling the eating cycle, such as that it is controlled by centres, one tending to produce and the other to inhibit eating, being affected by feedback from the animal's blood. It proved possible to identify an area in the brain which, if destroyed, would result in the animal wilfully starving to death, while stimulation can cause excessive eating.

Other sorts, terms such as 'purpose',

'desire' or 'hunger' have been re-defined, or restricted to a descriptive use, or have disappeared altogether, giving a progression remarkably similar to those which have occurred in sciences such as Physics. The principles of explanation which are employed do not appear to differ in any important way from those in other sciences. It might be argued, however, that this example neglects an important difference, that Psychology has access to a source of information essentially private, from which understanding of a scientific order, not otherwise accessible to us is derived. As Braithwaite puts it: 'The physical and the ordinary biological sciences use only public data open to observation by all, but the psychological and social sciences may also use private data of immediate experience.' This assertion requires examination. It leads to the question: What becomes of the term 'sensation' as psychological explanation develops?

The belief in a special and private subject matter has deeply influenced some of the investigations of experimental psychology, notably in regard to the problem of the 'measurement of sensation'. This field is of particular interest since it provides the simplest situations in which it might be supposed that we are studying private data not accessible to other sciences, and the only ones in which the effect of this assumption can be defined sufficiently exactly for some evaluation of it to appear possible. The consequences of applying the hypothesis of 'private data' to these experiments will now be considered.

One of the earliest discoveries about sensory functions was Weber's finding that the difference between two stimuli of different intensit that was just noticeable to a subject was an approximately consta proportion of the stimulus intensity. If the subject could reli distinguish weights of 51 and 50 grams, then he would be ab discriminate weights of 100 and 102 grams at the same level of ac Weber's law can be expressed as $\Delta I/I = k$, where ΔI is the ju able difference (according to some criterion), I is the stimul and k a constant. The first attempt to 'measure sensatio by Fechner, who used Weber's law for this purpose. assumption that for all pairs of stimuli on a particular could just be distinguished from one another the the sensations produced by the two stimuli in eac same. Thus the feelings of pressure caused by grams would differ by one subjective unit, a caused by 100 and 102 grams. It follo magnitude of a sensation co

give an apparent halving in sensation. In each scaling procedure the subject's responses are used to identify pairs of values of I meeting a criterion, and each value of ΔI is taken to correspond to a value of ΔS. In the one case it is supposed that $\Delta S = k$, in the other that $\Delta S/S = k$. But these two assumptions are arbitrary: there is no operation that will distinguish between them, so that they are no more than alternative definitions. If the first is used the function that results is $S_f = k\log I$, if the second, $S_d = kI^n$.

This difference does not, therefore, result from a difference in the data given by the two experimental procedures. If, ignoring any hypothetical 'unobservable operations' on sensations consequent on the instructions given the subject, the two definitions of ΔS were interchanged, then the scale given by fractionation would be a logarithmic function of stimulus intensity, and the discriminability procedure would result in a power function. Thus the two mathematical descriptions are completely equivalent.

An analogous physical situation would be provided by a device built to select series of lights of graduated brightness. If we examined such a series and found that it consisted of lights of 10, 100, and 1000 millilamberts in that order, then nothing in this observation would tell us whether a photometer measuring brightness in millilamberts was employed, and successive lights were selected at a constant ratio of separation, or whether the photometer measured log millilamberts and the device selected the lights at a constant interval. We would have no operation that could distinguish between these descriptions.

We have, then, two procedures giving different functions for the measure of sensation. But in the situations which have been discussed the measures are equivalent; one can be transformed into the other, and there is no difference in their observable consequences. They are distinguished only by the assumptions made in deriving them, i.e. that $\Delta S_d = k$ or that $\Delta S_d/S'_d = k$. In relation to the operations performed and observations made by the experimenter the choice of one of these assumptions is arbitrary. That they have not been explicitly by their proponents must imply that some criterion of this sort has been employed.

An argument has been proposed to justify Fechner's assumption. Possibly a naive expectation that sensation is limited by the absolute difference between stimuli in Weber's findings, and it may have then been, in terms of which the

discrimination was ' really ' made, instead; but this has not been explicitly proposed. In contrast, the choice of assumption for the direct methods is based on the belief that subjects report their sensations directly. This is most clearly expressed in Stevens's and Davis's account of the ' direct methods ': ' the response of an observer who says " this is half as great as that " is one which, for the purpose of erecting a subjective scale, can probably be accepted at its face value.' This seems closely related to the ' private data ' hypothesis. It could be imagined as the conclusion of an argument which might run:

> We recognise that there is a difference between a patch of red and a patch of green which we could not explain to a blind man. Similarly we might say that one noise *sounds* louder than another, or that Jones *looks* smaller than Smith though, we might add, he is really somewhat taller, but is standing considerably further off. Here, however, we are meaningfully comparing sensations, by referring to their magnitude. Perhaps we can use such comparisons to estimate the relations between such subjective magnitudes; if subjects can make consistent numerical estimates of these subjective magnitudes, scales could be derived from these estimates.

Some of the scientists concerned might object that they have not put forward such an argument, and are, perhaps, operationists or behaviourists. It is difficult, however, to see how a conclusion of this sort could be reached except from some such basis.

The implications of treating the subject's responses in a scalin experiment as reports can be made clearer by comparing them w scientific observations. The basis of science consists of report records of observations. These are made in accordance with cedures which may be more or less explicitly prescribed; an e feature is that they are not in principle limited to particular o Other scientists could have made them, and can attempt to confirm them. In a scaling experiment these primary would be the stimulus magnitudes and the subjects' r there is here an added complexity: the subjects' re form of observational reports. This presents a te of disregarding this similarity and following the science, i.e. attempting to devise a theory from wh would follow as consequences, they can be tre fact reports, from fellow scientists, but This is, of course, the procedure which justifies.

There are a number of difficulties with this procedure. By t... the subject's response as a report, but not a report about the stim... claims are being made for it which cannot be justified. Part of the definition of an observational report in science is that a procedure can be laid down which must be correctly followed for the result to constitute such a report. But no procedure can be meaningfully prescribed for the subject to follow in making a 'report' on private data: it cannot be said that he has adhered to the correct procedure or strayed from it. Furthermore a report of an observation can be correct or wrong. This depends at least partly on the possibility of that observation being made or checked by some other observer, who can agree with or refute it. But this is excluded, in principle, when the data are private. If there is no operation, however, which could confirm or refute a statement it is doubtful whether it can be considered capable of being true or false. Thus if the subject's remarks are treated as reports of observations this does violence to the criteria which properly define such reports, and has the effect of creating a shadow replica of the public world, contained in the subject and inaccessible to others.

However, even if this were not so, a second diff... would remain. It is usually accepted that measurement is a ... assigning numbers to objects or events according to rul... ible set of operations must define an observation f... emerges. Numbers do not inhere in objects ... intuitively discovered. It follows that judgn... f magnitude can only be made consistently ... following some rule and subjects will be co... if they follow similar rules. Since suffic... en subjects to allow scales to be derive... rocess of tutelage ... t to assert ...

choice. It is true that the two functions are equivalent in that we can equally well derive either from the same data. However a model can be suggested for the processes underlying performance in scaling experiments which provides a criterion for selecting one of the functions rather than the other. There is not sufficient space to give the argument in full, but its main points will be summarised.

In a scaling experiment the subject selects responses to stimuli presented to him. Since in doing this a measure of the input reliably determines a related output he acts as a computer of sorts, and the processes underlying response can be considered to correspond partly to an initial measurement and partly to a subsequent computation. If we were investigating a computer of familiar type we would expect more errors and failures to occur when long or complex calculations are required than when they are short and simple. Some tasks might require a complex computation if a measure derived from a log function were used, but a simple transformation of a power function, or vice versa. If the computer performed well when one of the functions required simple operations, and badly when complex transformations of that function were necessary, this would be a reason for describing [illegible] that function. One function would not have [illegible] provide a more correct description that the other, [illegible] ference in correctness between the geocentric or [illegible] f the solar system. Nevertheless, just as the [illegible] an inertial to an accelerated frame of reference [illegible] account in which the simplicity or complexit[illegible] ngruent with the apparent simplicity or co[illegible] r's, or the subject's, performance. By set[illegible] of problems and observing its perfor[illegible] ht allow us to asses[illegible]

stimulus magnitude, and the numbers given as responses are linearly related to this measure. But it also allows the alternative description in which the measure initially taken by the subject is a logarithmic function of the stimulus magnitude, and an exponential transformation of this measure determines the response. The fact that subjects often find magnitude estimation surprisingly difficult—some cannot do it at all—would slightly favour the more complex description.

Before discussing a more complex example we must shortly consider the problem of how subjects learn to use numbers or measures in dealing with objects or stimuli. The probable starting-point for this process of learning lies in the young child's need to cope with his environment. He is repeatedly confronted with stimuli of varying intensity—objects at different distances, changes in illumination, sounds loud and faint—and he must acquire the ability to deal consistently with them. He must become able both to predict changes in his environment, and to anticipate the effects of his actions, with some degree of accuracy. In this respect different stimulus dimensions will make different demands. When he is dealing with objects varying in length, for instance, he will need to be able to tell from inspection of two objects separately what their length will be when they are placed together. We make judgments of this sort so frequently, skilfully, and with so little hesitation that we may overlook that our ability to do this must be accounted for. It implies that some aspect of the effect of the stimulus or object on the visual system is related to its length, and is used as an initial measure of this length, and that some computation must be performed on the measures indicating the size of each object alone from which their probable length when placed together can be predicted. A computation of this sort would be difficult to perform with measures logarithmically related to the physical length, whereas if they were linearly related to it, and an operation equivalent to addition were available to the 'neural computer', it would merely need to learn to employ it. But if the neural measure is a logarithmic function of the physical stimulus, then before addition can be performed it must exponentially transform this neural measure. Measures for other dimensions might be determined by similar considerations, or by correlations between dimensions. Thus it might be of advantage to be able to relate loudness or illumination to the distance of a constant source. If we wish our measure of apparent loudness to double when the source doubles this would require

magnitude estimates of loudness to be a power function of stimulus intensity; if in this case the initial effect of the stimulus were given by a logarithmic function it would have to be exponentially transformed. If for a particular stimulus dimension there were no environmental pressures tending to bring about some transformation of the initial measure, then none would occur, and we might expect subjects operating on such a dimension to fractionate and make other judgments in terms of the untransformed logarithmic measure.

This account of the effect of learning on the operations and transformations performed by the computer which serves as our model for the processes by which the subject selects the correct response to a stimulus has been presented as though the initial measure of the stimulus were given by a logarithmic function. But, as we have seen, it could equally well be described as a power function. In this case the subject's experience would have the same effect in determining the operations he comes to perform, but they would be described differently.

We can now consider a complex finding which allows us to compare these two descriptions. This is that while some dimensions, such as loudness, give power functions when scaled by the direct methods (on the assumption that $\Delta S_a/S'_a = k$), and logarithmic functions when the indirect methods are employed (with the assumption that $\Delta S_f = k$), this is not true for all dimensions. With pitch, for example, the same scale is obtained by fractionation and by summating j.n.d.s as equal units. How is this to be explained?

We have seen that the relation between the intensity of a sound an
the distance of its source might be sufficiently important to determi
an optimal measure into which an inadequate initial measure of
stimulus might come to be transformed. No relation of equi
importance appears to attach to pitch. If we choose the log
function to describe the measure constituted by the initial eff
auditory system of sounds of varying loudness, then we
loudness discrimination is determined by the interval of
this scale, the j.n.d. corresponding to a constant interval
of the correlation between loudness and distance, th
by 'direct' procedures such as magnitude estim
operations on the scale given by exponentiall
initial measure so as to make it approximate
stimulus intensity with an exponent of appr
which have been experimentally dete
between 0·3 and 0·5). With pitch ther

to learn a transformation of the initial measure, on which $\Delta S_f = k$ corresponds to the j.n.d.; when, therefore, he is asked to fractionate pitches he will use this scale to do so, so that both fractionation and summating j.n.d.s will produce the same scale.

If we wish to use the power function as a measure of the initial effect of the stimulus the same causes—the presence or absence of environmental pressures favouring particular sorts of learning—should explain the subjects' performance. But the description will change. With loudness, magnitude estimates will be a linear function of the initial power function, and discrimination can be referred to the power function too, on the assumption that the j.n.d. corresponds to $\Delta S_f S_f = k$. The simplicity of this description, in which there is only one scale, presents a difficulty: where can learning be said to have had its effect? There is no disparity between scales to be explained. In order to include the effect of learning in this new account we must make assumptions such as that the simplest relation between magnitude estimates and the initial measure is for the former to be a logarithmic function of the latter, and that the effect of learning is to replace this simple relation by the more complex linear function. For pitch too we can assume that the j.n.d. corresponds to $\Delta S_f/S_f = k$, and derive the corresponding scale, which will be equivalent to an exponential transformation of the summated j.n.d. scale. But fractionation will not result in a power function: instead it will produce the summated j.n.d. scale which we have avoided deriving from the discriminability data. Here, where we found no environmental pressure that might lead the child to learn a complex transformation we now have two different functions. To explain this we shall have to claim that the best way for the computer to fractionate is for it to take a logarithmic transformation of the initial measure and fractionate that; and the absence of any environmental pressure that might lead the child to learn the more complex procedure of fractionating the initial measure. Correctly, the simple computation has been performed.

Before the descriptions and explanations which can be given for the two functions it is clear that we should normally prefer the logarithmic function. The problem which has been solved by a number that arise in scaling experiments and which show the power of simpler and more plausible explanations when a power function is applied. But the main weight of the arguments in their leading us to use a function other than the 'private data' hypothesis, as in

that they present a coherent alternative account, based on a theory of the sort familiar to science, to which the observed phenomena can be logically related. Not merely is the ' computer model ' an alternative to the ' private data ' hypothesis, and, in contrast with the latter, consistent with the canons of scientific theory-building, but it also shows phenomena to be explicable for which the latter can offer no adequate explanations. When there are two alternative theories in a given area it is usual to prefer that which satisfactorily explains more of the data; one thing we cannot do is to hold them both.

In this discussion of sensory scaling we have seen that the hypothesis that ' private data ' form part of the subject matter of science led to the power function being preferred to the mathematically equivalent logarithmic function. But the adoption of this hypothesis involved considerable difficulties. It required that statements by subjects should be treated as though they were reports of observations though they did not meet the operational criteria for such reports. It also required the assumption, not otherwise acceptable, that numbers or measures inhere in data and are discovered intuitively. Finally a model was discussed which accounts for some of the features of subjects' performances in scaling experiments, and leads us to find it natural to use the logarithmic rather than the power function. In contrast to the ' private data ' hypothesis this model satisfied the usual requirements for a scientific explanation; it also appeared to explain phenomena for which the former offers no explanation. For these reason it appeared that the ' computer model ' should be preferred and th ' private data ' hypothesis rejected as an explanation for subje performance in scaling experiments.

In considering the results of attempts to ' measure sensatio have found that the common associations of the term ' se most clearly expressed as the ' private data ' hypothesis, did in developing explanations in this area. Instead difficult which led to its replacement by a theory of a different so that with ' sensation ', as with ' purpose ', or ' solid the progress of scientific explanation is marked l utility and eventual dropping of the familiar te true that only one area of investigation has been possible that a different result might have b area. However sensory scaling does appe which fairly exact implications can be a hypothesis.

composite ones, and that it explains their behaviour. Lavoisier's demand, that it must be possible to isolate any substance assumed to be a chemical element, i.e. to prepare it as 'simple substance' with definite properties, has proved very apt, since it is possible to prepare most elements in the pure state without too much difficulty. For a proper appreciation of this principle from the philosophic point of view, however, it is very instructive to consider the exceptions.

In Lavoisier's first table of elements we find 'light-substance' and 'heat-substance'. These had not been obtained as simple substances, and certainly Lavoisier did not believe even in the theoretical possibility of preparing them in the pure state. They were principles for the explanation of certain properties, and it would be difficult to state any essential way in which they differed from the principles of the alchemists or the old element fire. Soon, advances in physics caused light and heat to be deleted from the list of substances, and thus also from the chemists' table of elements. But even after the chemists had confined themselves to ponderable substances, the decision as to which should be included in the table required more than just a schematic application of the new definition, if the value of the concept of element was not to be considerably reduced. Thus Lavoisier allowed a place amongst the simple bodies to the chemical compounds lime, magnesia, baryta and alumina, which had not been decomposed at the time, although he considered that they were probably oxides; but he omitted potash and soda, because he was already firmly convinced that they were composed of basic constituents still to be discovered. This was almost twenty years before Davy succeeded in obtaining the new metals potassium and sodium from them. Here, then, Lavoisier himself assumed hypothetical elements; these had no more been produced than, say, the purest earth of the Aristotelians, or the special sulphur of the Spagirists (which is not the same as common sulphur). His translator, Hermbstädt, wanted to avoid the uncertainty which lay in the indirect inference by analogy to elements, contrary to the principle of pure empiricism just put forward: he put potash and soda back into the table.[1] However, it was not his pedantic logical consistency, but Lavoisier's happy instinct, that proved to be right.

The inferring of elements which cannot be prepared must not be regarded as in any way merely a necessary imperfection of the initial

[1] A. L. Lavoisier, *Traité élémentaire de chimie*, German translation by S. F. Hermbstädt, Berlin, 1803, p. 247, footnote.

stage of chemistry. Later, too, elements were acknowledged with complete certainty, though their preparation as simple substances had not been achieved. Thus nobody doubted the existence of fluorine even yet before the famous work of Moissan, since one could not explain the special properties of hydrofluoric acid and its salts with the known elements alone, and thus needed a new ' principle '. Indeed, after the discovery of chlorine, arguments from analogy led to fairly clear ideas about the nature of this radical of hydrofluoric acid—which is already named amongst the simple substances in Lavoisier's first table. If one wishes to cite a very recent example of this procedure: the preparation of radium as a metal was a great experimental success of Madame Curie, but without fundamental significance; radium had already obtained its assured place in the system of elements, because the assumption of a new basic substance was necessary to explain the radioactive properties of its salts.

If we accept the legitimacy of this procedure, leading, in the instances quoted above, to the postulation of the existence of particular basic substances, we must not raise any fundamental objection to the inferences which were used to justify the conception of the medieval elements. For example, the statement by which Stahl justified taking aether, water and earth as *principia prima* also applies *exactly* to the cases cited here: *Simplicia corpora et elementa, quamvis non pura et separata dentur, realiter tamen distincta a mente possunt concipi.*[1] Sodium, fluorine, and radium could not be isolated in the pure state either at the time when they were introduced into science as basic substances, but could only be rationally inferred. However, the vast chemical experience gained in the meantime, and the guidance of analogy, made inferences incomparably more certain in these cases.

Thus we consider the great advance due to Lavoisier to consist in this: he did not limit the number of chemical basic substances *a priori*, but he demanded that they could *in principle* be prepared as non-decomposable substances. Further—and this is the point where he went beyond Jung and Boyle—he gave a means, the balance, of proving a substance to be non-decomposable. However, as is shown particularly clearly by those cases in which the demand for preparability was dropped, the ' metaphysics ' of the basic philosophic idea of the concept of element is not touched by this.

[1] G. E. Stahl, *Fundamenta Chymiae dogmatico-rationalis et experimentalis*, Nürnberg, 1732, p. 1

5 *The Double Meaning of the Chemical Concept of Element: 'Basic Substance' and 'Simple Substance'*

Admittedly it seemed as if Lavoisier had changed the philosophic character of the concept as well. His elements seemed—as one can read in numerous books—no longer some sort of metaphysical causes of the behaviour of bodies, as with the alchemists, but visible and tangible substances with quite definite observable properties.[1] Indeed, Pattison Muir, the editor of a new edition of the *Sceptical Chymist*, even wishes to read an analogous view into Boyle, who, as a declared adherent of atomism, should have been safe from such misinterpretations. Muir considers that Boyle was well on the way to the pragmatic point of view that an exact science must not assume an invariant substratum devoid of qualities, but must regard a substance only as the sum of its properties which are perceptible to the senses.[2] To the question: 'what is the element sulphur?', Geber's answer might have been: the carrier of mutability by fire. The present-day chemist describes the element sulphur as a substance of pale yellow colour, without taste or smell, insoluble in water, etc. All these are its properties. How is it that it does not retain them in its compounds? What sense at all is there in saying that the element sulphur is preserved unchanged in its compounds, such as the gaseous, colourless, pungently smelling sulphur dioxide?

[1] To quote only one of the more recent authors: "Vergleicht man nun die Elemente Boyles mit denen des Aristoteles und den Prinzipien, so erkennt man leicht, dass sie begrifflich völlig von ihnen verschieden sind. Unter den alten Elementen versteht man verkörperte Eigenschaften, unter den neuen in der Natur vorkommende Stoffe." (A. Benrath, *Chemische Grundbegriffe*, Berlin, 1920, p. 26.) [If one compares Boyle's elements with those of Aristotle and the Principles, one soon realises that the former are conceptually quite distinct from the latter. The old elements are to be understood as embodied properties, the new elements are substances occurring in nature.] [Note by translator: cf. also J. R. Partington, *A Short History of Chemistry*, 2nd edition, 1948, p. 67: "'. . . he [Boyle] gave a clear definition of an element and showed by experiment that the four elements of Aristotle and the three principles of the alchemists (mercury, sulphur, and salt) did not deserve to be called elements or principles at all, since none of them could be extracted from bodies, e.g. metals."]

[2] *The Sceptical Chymist* by the Hon. Robert Boyle, Everyman's Library Edition, p. xviii. Similarly, it will be clear from the above argument that we reject completely the view expressed by M. M. Pattison Muir elsewhere (*The Alchemical Essence and the Chemical Elements*, London, 1894) that there is an unbridgeable opposition between alchemy and chemistry.

What has been said in the previous section should make our meaning clear immediately, when we say that the element sulphur represents today, no less than in the old days, a transcendental principle underlying the phenomena; and that the mistake in the statements cited above lies in the naive-realistic assumption that the properties of sulphur are just those properties which it exhibits to us in the special case when it is not combined with any other basic substance. Admittedly, as has been explained above, we may, in considering a particular substance, speak just as well of its properties as of the properties we perceive in it. But Lavoisier's definition must not mislead us into applying this simplification to the concept of element, which is used in *two different senses*. I have already attempted to keep apart these different meanings in the above discussion by using the two terms 'basic substance' and 'simple substance'.[1] I have referred throughout to '*basic substance*' whenever *the indestructible substance present in compounds and simple substances was to be denoted*, and to '*simple substance*' whenever *that form of occurrence was meant in which an isolated basic substance uncombined with any other appears to our senses.* We cannot ascribe specific properties to an element in the sense of basic substance, since the latter contributes to the generation of the infinitely diverse properties which it exhibits, by itself and in combination with other basic materials; as simple substance it can be characterised by the statement of its properties without prejudice to scientific exactness, as we have earlier shown in general for any substance. In the case of the concept of 'simple substance' we may remain naive realists; but in the case of 'basic substance', if we are not to get involved at once in contradictions, we must not overlook the fact that it belongs to a transcendental world devoid of qualities.[2]

[1] Many textbooks do not use the term 'Grundstoff' [basic substance] at all, others define it at one point as synonymous with chemical element, but use it no further. It has, at any rate, not been needed to specify concepts until now, and may therefore be used with a new significance.

[Translator's note: throughout this article, we have used the terms 'basic substance' and 'simple substance' as translations of the German 'Grundstoff' and 'einfacher Stoff' respectively.]

[2] It goes without saying that, in the case of simple substance also, any closer consideration is bound to lead us to the well-known contradictions of naive realism. We need only think, for instance, of the change in properties of the simple substance sulphur on transition from the rhombic into the monoclinic form, and even more on transition into the gaseous state; or again, of the impossibility of explicating potential qualities, such as solubility or combustibility, in this view of the world.

Even to this day the customary definition follows Lavoisier: "an element is a substance that cannot be decomposed into simpler ones by any chemical procedure."[1] Thus, to be sure, the assumed *criterion* for an element is that it may be obtained as a simple substance which cannot be decomposed further. But the great *significance* of elements for the whole body of chemical theory lies in the assumption that the same substances which produce the phenomenon of 'simple substances' serve in the quality-less, objectively real sphere of nature as 'basic substances' for the construction not only of simple but also of composite substances. *The fundamental principle of chemistry that elements persist in their compounds refers to the quality-less basic substances only.* In any other sense the statement is unintelligible. Ostwald's interpretation of the principle to the effect that there is the "possibility of regaining the element in unchanged amount from every one of its

In general, it is true, when we consider the simple substances of chemistry, these contradictions need not obtrude into our consciousness, any more than they do in the case of objects of daily life; they are in fact ignored in the usual laboratory practice.

[1] This restriction to chemical procedures in the definition of chemical element is appropriate today after the discovery that nitrogen, aluminium, and several other elements, may be disintegrated—not indeed by chemical means, but by the impact of alpha-rays—and that most elements are mixtures of two or more chemically inseparable isotopes. Another modern definition—to be preferred for several reasons—runs: 'A chemical element is a substance all of whose atoms have the same nuclear charge.' This definition differs from the conventional one in its more theoretical tone. But here, too, the possibility of preparing the simple substance is demanded as a necessary condition. For this reason we need not here consider more closely the advantages and disadvantages of this definition. (On this point, see *Zs. f. phys. Chem.* 1916, 91, 171; *Naturwissenschaften*, 1918, 6, 646; 1920, 8, 839.)

[Note by translator: In these early publications, the author advocated that the term 'chemical element' should continue to denote such substances as lead and chlorine, even though these substances were then known to consist of several isotopes of different atomic weight, and although some of these isotopes could be separated. Against the view of a number of chemists, he held that, from the chemical point of view, it was better to drop Dalton's principle that there were as many elements as atomic species, rather than abandon the chemical classification of elements in the periodic system. His argument involved a distinction between 'physical' and 'chemical' (including electro-chemical) methods of analysis, and is rooted in the experience of early experiments establishing the practical 'impossibility' of distinguishing between isotopes by chemical means. These experiments led to the method of radioactive tracers.]

It is only in the formulation: 'A chemical element is the class of all atoms of equal nuclear charge' that the chemical element would be defined with complete generality as basic substance, and no longer as simple substance.

compounds "[1] is no way out, for, if that is all that is meant, one should not speak of " persistence in compounds ". One is forced into such a re-interpretation, and so to an abdication of any understanding of the phenomenon, if one assumes that " any substance, and hence any element, is characterised by the sum of its properties ". Probably no other example shows more strikingly that the simplest and most familiar scientific laws are only valid under the implied or explicit assumption of a transcendental world.

If it is maintained that the elements are not present in their compounds in actuality (as basic substances), but only potentially (as simple substances), then the fact that the properties of their compounds can form a basis for the most profound systematics of the chemical elements becomes inexplicable. As is well known, the groups of the ' natural system of elements ' indicate the similarity of the properties of compounds to a greater extent than that of the properties of the simple substances—we need only recall how little resemblance there is between gaseous nitrogen and pure antimony. Thus, in terms of the distinction introduced here, we may refer only to a natural system of basic substances, not of simple substances.[2] Perhaps it is the many relationships between the forms of combination of chemical elements, expressed by the natural system, that most clearly reveal that, behind the ' simple substances ' and ' compounds ' endowed with constant properties, there stand the transcendental ' basic substances ', producing Proteus-like, innumerable forms, yet nevertheless bound by rigid rules; and that it is just the task of chemistry to discover the laws of this transcendental world.[3]

[1] W. Ostwald, *Vorlesungen über Naturphilosophie*, Leipzig, 1902, p. 287. Similarly W. Ramsay, loc. cit. p. 148

[2] Cf. D. Mendelejeff, *Grundlagen der Chemie*, German translation by L. Jawein and A. Thillot, Petersburg, 1891, p. 688. [Cf. *Principles of Chemistry*, 3rd English edition, London, 1905.]

[3] The atomic theory of Rutherford and Bohr enables us to visualise particularly vividly how we are to understand the persistence of an element in its compounds; namely, as the unchanged presence of all atomic *nuclei* of the simple substance. The simple substance with its optical, chemical, etc. properties has disappeared because the *electronic shells* of its atoms have undergone changes. (Only two of its properties, weight and—where present—radioactivity, are unchanged, because they reside in the atomic nucleus.) The simple substance can, however, always be recovered, with all its properties, as long as the atomic nuclei are not destroyed, because in this case the electrons can be brought back into their original positions, i.e. the positions which they occupy in the absence of alien atomic nuclei. (Significantly, clarification is obtained here through the notions of physics, a science whose concepts do not have the naive-realistic coloration of the chemical concept of element.)

Our present day chemical language does not, in general, reflect the difference between the meaning of 'basic substance' and 'simple substance' in the nomenclature of the elements. Only in a single case, that of carbon, are different names actually used in German, depending upon whether we are referring to the basic substance or the simple substance; and even here the distinction is not carried through strictly. The term carbon [*Kohlenstoff*] usually denotes the basic substance only, while the simple substance is referred to as charcoal [*Kohle*]. We speak of the carbon- (not charcoal-) content of organic compounds, but of the properties of charcoal (not of carbon). Admittedly this distinction is not always observed. Thus the shorter name *Kohlendioxyd* is frequently used instead of *Kohlenstoffdioxyd*, and, on the other hand, we speak of amorphous carbon instead of amorphous charcoal. At any rate, we find here a distinct beginning of a linguistic separation of the two meanings, favoured in this case by the fact that the free element occurs in different allotropic forms (charcoal, diamond, graphite); in this way the difference between the basic substance, common to all of them, and its forms of occurrence as a simple substance, is made particularly apparent. For this reason a new artificial term [*Kohlenstoff*] was introduced to denote the basic substance. This name was, to be sure, derived from the name of the most common form [*Kohle*] of the free element, but not made synonymous with it. However, in another case where two different forms of the free element exist, the name of the more abundant form is used for the basic substance also; 'oxygen' [*Sauerstoff*] is the name of the basic substance of both the gas called oxygen and of ozone. It would certainly be helpful to have a special name for the gas oxygen, as well as for ozone.[1, 2] However, I do not, of course, wish to suggest by this a linguistic reform impossible to carry out so late in the day, but only to point out what would seem to be indicated in the interest of logic. Much confusion could thus be avoided. Take, for instance, the following sentences, correct according to present chemical usage: 'Copper oxide consists of copper and oxygen,' 'Copper is a metal; oxygen is a gas'. Therefore, one must conclude, copper oxide 'consists' of a metal and a gas, which is obviously nonsense. The fallacy here is based on the use of 'copper' and 'oxygen' in the sense of basic substances in the first sentence and in the sense

[1] Cf. D. Mendelejeff, loc.. cit. English edition, Vol. 1, p. 207, footnote 17.

[2] [Note by translator: I have omitted here a remark to the effect that it would have been desirable to reserve the end-syllable '-stoff' in German for basic substances.]

of simple substances in the second. Thus we have a '*quaternio terminorum*' made possible by linguistic identity.

To sum up: using the two terms, we may say that the important advance in Lavoisier's definition of element lay in inferring the existence of a basic substance from each simple substance found experimentally.[1] An accompanying disadvantage was that, in the eyes of many, the meaning of 'basic substance' was completely hidden by that of 'simple substance'; the close relationship of the modern concept of element, also, to the alchemists' principles which underlie appearances, was obscured, and the principle of explaining the φαινόμενα on the basis of the ἄδηλα, already found necessary by the Greek philosophers, was abandoned as unscientific metaphysical speculation.

Of the two senses in which the expression 'chemical element' may be used, philosophical writers of today usually consider only one: 'basic substance'. Accordingly we find the concept of element subordinated to the concept of substance, and its 'metaphysical' character emphasised. In chemical textbooks, however, this abstract meaning has been lost, precisely because of Lavoisier's influence, and elements are understood to be not devoid of qualities but substances with certain definite properties. "With reference to the elements" the chemists, as Wundt justly says, have in many cases "not got beyond the concept of thing derived from common experience".[2] I have found only two instances in which the two different meanings of the term 'chemical element' are pointed out. In the article 'Elements' by C. Hell, in the *Neue Handwörterbuch der Chemie*,[3] it is emphasised that this may mean the "simple substances" as well as the "material constituents of the simple and composite substances themselves". "According to the second definition", the article continues, "the concept of element coincides with that of atom, and serves mainly to designate and individualise the latter more closely"; "the atoms are the true elements of bodies", a statement

[1] For the sake of simplicity we ignore here the possibility that a simple substance may occur in allotropic forms.

[2] W. Wundt, loc. cit. I, p. 484. Compare with this the standpoint already reached by 'Basil Valentine' (*circa* 1600), who distinguished between salt, sulphur, and mercury as preparable substances and as 'seeds'. This latter term coincides with what we refer to as metaphysical principles; for instance, in the passage: "Er ist der sichtige Mercurius metallum, ein Corpus vor sich selbst, aus denen sieben Corporibus, kann derhalben kein Saamen seyn." (H. Kopp, *Beiträge zur Geschichte der Chemie*, III, Braunschweig, 1875, p. 126.) ["The visible metallic mercury is a body in itself, out of those seven bodies, and therefore cannot be a seed." For a discussion of the complex theory found in the writings of 'Basil Valentine' (of questionable identity) see H. Kopp, ibid. pp. 124-127].

[3] *Neues Handwörterbuch der Chemie*, Braunschweig, 1878, Vol. 3, p. 1

which is not, in my opinion, correct. The atomic theory can, it is true, contribute enormously to—indeed, may be necessary for—visualising how the basic substances persist in simple substances and compounds;[1] but the concept of basic substance as such does not in itself contain any idea of atomism. It was, after all, while explicitly rejecting atomism that Lavoisier carried this concept to victory; and also in more recent times, there were, and are, chemists who avoid the atomic theory but retain the elements, including, of course, elements in the sense of basic substances.[2]

The distinction is also found in Mendelejeff's textbook of chemistry. Mendelejeff emphasises very forcefully the difference " between the concept of a simple body as a single homogeneous substance, and as the material constituent, not perceptible to the senses, of a composite body ".[3] Only in the latter sense does he call a simple body an " element ". I can think of only one reason why his very justifiable distinction, and his other passages based on it, which I have quoted earlier, have not produced any echo in later chemical textbooks;[4] namely, his inapt terminology. ' Element ' and ' simple body ' were introduced by Lavoisier explicitly as synonyms (cf. also the expression ' elementary sulphur ', customary today, by which is meant sulphur as simple substance), and it is thus hardly possible to use them to designate opposites. For this reason I have preferred to speak of *basic substance* and *simple substance* as *different aspects of the chemical concept of element.*

[1] Cf. Aristotle's view, quoted above (note 2, p. 145), concerning the relation of the Greek atomists to Empedocles. Jung, who wanted to replace the ' actual-potential ' doctrine (see p. 146 above) by that of ' syn-dia-crisis ', i.e. atomism, and Boyle saw the inevitability of the process of thought leading from the assumption of constant elements to atomism as clearly as did Aristotle. Lavoisier, on the other hand, felt himself to be an opponent of " metaphysical " atomism when introducing the very same concept of element.

[2] Besides the German works written or influenced by Ostwald, we may mention here the " deliberate exclusion of all atomistic ideas ", presumably a conscious carrying on of the Lavoisier tradition, in one of the best-known French textbooks. (Passage quoted from Haber's preface to the German translation of Le Chatelier, *Vom Kohlenstoff*, Halle, 1913.)

[3] D. Mendelejeff, *Grundlagen der Chemie*, 1891, p. 27 (cf. *Principles of Chemistry*, 1905, p. 23). Also, previously, *Ann. d. Chemie u. Pharm.*, 1872, **8**, Supplement, 133

[4] Only Meyerson approaches the two views just quoted, in a passage where he speaks of the ' atomic element ' (*Identité et réalité*). [English translation, *Identity and Reality*, Library of Philosophy, London, 1930, p. 338.] However, his reference to atoms in this connection seems to me as unfortunate as in the passage by Hell discussed above, and to miss the essential point. For, on the one hand, we may take also groups of atoms and molecules in the transcendental sense (cf. our comments on radicals below), and on the other hand, there are simple substances too, which are built up of atoms and not of molecules, as, for instance, the inert gases, metal vapours or active hydrogen. Incidentally, Mendelejeff, too, is inclined to identify element with atom (*Principles of Chemistry*, Vol. 1, p. 207, n. 17).

6 *The Double Meaning of Other Chemical Concepts*

Our discussion would be incomplete if we did not, in conclusion, at least indicate that the distinction between basic substances devoid of qualities and simple substances perceptible to the senses, which we have applied in the case of the concept of element, is, in a very similar form, of importance in other problems of chemistry. It applies quite generally whenever we speak of the persistence of a substance under chemical change.

The 'radicals' of organic chemistry, for example, exist almost without exception in the world of the transcendental alone.[1] The opponents of the theory of radicals have always demanded, before they could believe in them, that radicals be prepared as observable substances. But this demand we must consider unjustified, for the same reasons adduced above to show that the existence of a chemical element as 'basic substance' may be completely proved without its having been isolated as 'simple substance'.[2] Radicals are, in fact, an even more convincing example of the transcendental character of the laws of conservation in chemistry; for—in sharp contrast with the case of the elements, the majority of which can be prepared easily as simple substances—the old proofs of the independent existence of free radicals, perceptible by the senses, were all false and were soon refuted; nevertheless the assumption of radicals was completely justified. Once the attempt to prepare radicals had been given up, their use as chemical principles devoid of qualities could be developed in full measure, without danger of epistemological obscurities and misunderstandings.

The situation is different when we consider the statement that the so-called 'compounds of first order', e.g. sulphur trioxide (SO_3) and water (H_2O), combine into 'compounds of higher order' such as sulphuric acid ($SO_3 . H_2O$), for here we know the compounds of first order as perceptible substances. After what has been said above it should be clear, however, that is is again only possible to assume their continued existence in the compounds of higher order as 'of

[1] We may ignore here the relatively very rare cases in which so-called 'free radicals' have recently been prepared.

[Note by translator: It might be mentioned here that short-lived aliphatic free radicals were first prepared and detected by the author.]

[2] Liebig pointed out this analogy long ago in a polemic against Laurent (*Ann. d. Pharm.*, 1838, **25**, 1).

transpose them, divested of their qualities, into a transcendental world. This example is, however, less clear-cut, for, from the chemical point of view, it is also possible to hold that the compounds of first order are not preserved as such, even in a transcendentally real sense, but rather that only the atoms contained in them are preserved, in a new arrangement. Sulphuric acid, say, would then be ' formed ' from SO_3 and H_2O, but would no longer ' consist ' of them (formulation: H_2SO_4). It is clear that examples are the less suited for discussion of logical principles, the less certain the scientific situation is with regard to them. For this reason we have confined ourselves in our discussion mainly to the undisputed law of persistence of the chemical elements.

Nevertheless we will consider one other, somewhat more complicated, chemical law of conservation, viz. the well-known law of chemistry that a salt is formed from acid and base with production of water. We will discuss this theory, which constituted a fundamental scientific advance, because violent objections were raised by philosophers when it first appeared. This led to an interesting controversy between one of the greatest chemists, Boyle, whose name has already appeared repeatedly in our discussion, and one of the most famous philosophers, Spinoza. This scientific polemic may be found in Spinoza's correspondence[1] of the years 1661-1663.

Boyle had recently demonstrated that nitre may be synthesised from an acid and a base; he therefore considered these two substances, endowed with completely different properties, to be the components of nitre. The secretary of the Royal Society, Oldenburg, had sent the paper in question to Spinoza, asking for his criticism. In two long letters to Oldenburg, Spinoza thereupon put forward his objections to the scientific standpoint of Boyle which corresponds completely to the view taken in chemistry even today. Spinoza is a Cartesian and admits only one single primary matter, whose corpuscles differ from each other only in shape, position and motion.[2] He censures Boyle

[1] [Note by translator: The author mentions two German translations: J. Bluwstein, Leipzig, Insel-Verlag, 1916, which he considers to be very abbreviated and unintelligible in parts, and J. Stern, Leipzig, Reclam, which he recommends. Passages probably corresponding to some of the quotations in the text may be found in A. Wolf, *The Correspondence of Spinoza*, London, 1928, pp. 110, 112, 130, 136-137. The word ' nitre ' is used as a synonym of ' saltpetre '.]

[2] A passage from a letter quoted by Moissan (*Traité de Chimie Minérale*, Paris, 1904, I, p. 1) shows that Descartes thought of the Peripatetic as well as the alchemical elements as being built up of like particles of primary matter.

for " putting on the same level those concepts which show nature, not as she is in herself, but in her relation to the human senses, and those which explain nature as she is in herself". As the latter he only admits " motion, rest and their laws ". He considered it so improbable that homogeneous nitre should be composed of two compounds so different in kind, as Boyle claimed to have proved, that he performed experiments especially to refute him; and indeed he believed he had found that it was unnecessary " to accept any difference between spirit of nitre [now called nitric acid] and nitre, other than this, that the particles of the latter are in a state of rest, whilst those of the former are in violent mutual motion. Nitre and spirit of nitre are related as ice is to water ". The solid basic residue, the ' calx of nitre ' he declared to be not an essential component of nitre, but an impurity.

We need not dwell on Spinoza's experiments. With the limited facilities at his disposal and his utter lack of practice in this field, he was unable to obtain useful results. It was only owing to certain errors—which Boyle, with his great experience, was able to point out immediately—that Spinoza believed that he had also been able to prove his views by experiments. Of interest to us is his firm conviction that Boyle's experiments and their interpretation must be false, because they do not correspond with his idea of the essential nature of scientific explanation. Here, at the beginning of scientific chemistry, we encounter, in the person of Spinoza, the first—but by no means the last—philosopher who will only admit the same concept to be valid in chemistry as in physics. We must admit that formally he is in the right, in that Boyle's statement that nitre ' consists ' volatile nitric acid and a solid residue shows just that *oscillation between the naive-realistic and the transcendental meanings of the terms* which we exhibited above as particularly characteristic of the concept of element. Nevertheless, it is surprising that Spinoza did not understand Boyle's epistemological point of view at all; for, though the latter nowhere explicitly points out this double meaning, he nevertheless discusses at some length the application of corpuscular theory in the explanation of chemical processes, even in the paper considered by Spinoza. These deliberations should have shown Spinoza how Boyle really pictured the persistence of chemical substances: he meant it in the sense of constancy, not of their qualities, but only of the structure of their particles in a transcendental world in which acids are not sour, bases are not alkaline, and salts not salty. In the deepest convictions of his

philosophy of nature he was no less a Cartesian than Spinoza. It is to be regretted that he never entered into direct exchange of ideas with him (probably his strong religious sensibility prevented him[1]); for we may well believe that he would have been able to dispel Spinoza's objections. He confined himself rather to giving Oldenburg his answers in the course of conversation:

> " He had not intended to give a truly philosophic and thorough explanation by his analysis of nitre." " He had never indertaken to discover the nature of nitre, nor to reject what anybody could teach concerning the homogeneity of matter and the difference of bodies arising only from motion, shape, etc. This only he wanted to show: that the various structures of bodies produce their various differences, and that from these proceed very different effects. Philosophers and others rightly inferred from this a certain heterogeneity, as long as reduction to one primary matter had not yet been achieved ".

Thus, Spinoza only admits into scientific research mathematically definable data about the particles of primary matter, and excludes all sensible qualities. Boyle, on the other hand, as emphasised at the beginning, recognises that this aim is unattainable for chemistry, and that an intermediate standpoint, which assumes substances of several kinds, may also be genuine science.[2] As Oldenburg wrote to Spinoza: Boyle belonged " to those who do not rely so completely on their thought that they believe themselves free to ignore whether or not it agrees with the phenomena of reality ".

" In the development of the concept of matter, science and philosophy have rarely been in agreement ", Wundt[3] says at one point, and here, if anywhere, their quarrel is understandable. Philosophy above all to the strict logic of its concepts, science to agreement perience ". The way in which Boyle and Spinoza respectively

[1] See A. Wolf, loc. cit.; review: *Nature*, 1929, **124**, 787

[2] It may be pointed out that Spinoza's conviction that one could not separate out two heterogeneous substances from nitre is not connected by necessity with his assumption of a basic matter as eternally persisting substance (against this, cf. the note by J. Bluwstein, loc. cit. p. 327). We must think of the different substances as arising out of this basic matter by different arrangement of the particles. Thus it is not the change of substances, but on the contrary the constancy of elements, that is difficult to explain if we assume a primary matter. Boyle, incidentally, accomplished this in a way that is still accepted in principle today. (On this point, see *Zs. f. phys. Chem.* 1916, **91**, pp. 171, 174; *Ergebnisse der exacten Naturwissenschaften* 1, Berlin, 1922, pp. 362, 367).

[3] W. Wundt, *Essays*, Leipzig, 1885, p. 59

interpret the phenomena in the case of nitre is typical of this difference in mental attitude.

The very thing Spinoza censures, the mixing up of those " concepts which show nature in her relation to the human senses, with those concepts which explain nature as she is in herself" has remained the well-founded procedure of chemistry to the present day. Thus, even today, to repeat Boyle's words also, it is still legitimate " to infer a certain heterogeneity from the different effects of substances, as long as reduction to one primary matter has not yet been achieved ". Indeed, we may add that even when one day this problem is solved, chemistry, as the science of the changes of substances, will still be justified in going no further than the reduction of the phenomena to the chemically indestructible substances, the elements, and thus in retaining qualitative differences in its fundamental concepts. The object of the present study has been the more precise definition and elucidation of the peculiar dual epistemological status of the concept of element, which is a consequence of this. As has been shown, this concept must be taken in the naive-realistic sense when meaning ' simple substance ', but has to be understood as transcendental when meaning ' basic substance '. We have also tried to emphasise the close relationship of the concept of basic substance to the ' metaphysical ' principles of the Aristotelians and the alchemists.

(concluded)

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DISCUSSIONS

On 'Space and Sense-data'

A Reply to Lord Brain

Brain [1] has recently published some arguments against the theory, to which he had previously subscribed, that perceptual space is a different space to physical space. Before analysing his development of these arguments there are two details of his presentation that require comment. Firstly he always refers to the spatial system of our immediate experience as 'perceptual space' containing 'perceptual objects' as distinct from physical space that contains physical objects. This usage is correct in this particular context as Brain is employing the distinction between perceptual objects and physical objects to develop arguments against Russell who had formulated his claims in this way. However he refers *only* to Russell's version of the multiple space theory. This is that sensible space is located in the physical brain. He does not mention the important versions of this theory introduced by Broad [2] and Price [3] where sensible space is held to be a different space to physical space and not merely a part of it. If we follow Broad and Price rather than Russell we must use the term 'sensible space' (containing 'sensible objects' or 'sense-data') instead of 'perceptual space'. This enables us to follow common usage and to say *that we perceive* physical things and that we 'sense' sense-data. Otherwise we have to say that we perceive our sensations which will lead us into all kinds of trouble.

This course of action will enable us to avoid some of the difficulties that Brain encounters, in particular the complications that is supposed to engender when I consider how I can lift a cup of tea to my lips (p. 181, the two-space theory is). These difficulties arise partly from using 'perceiving' by sight and touch instead of 'sensing'. For both we certainly do contact (perceive) physical objects and not perceptual objects. Incidentally, we do not see physical objects. It is thus valid against Russell's theory which is, quite Brain's cup-of-tea paradox is valid only against any theory that states that against any also a multiple space theory. Brain's argument is not valid, however, against It is multiple space theory that does allow us to perceive (see) physical objects. therefore not valid against Broad's and Price's theories. The paradox arises from the statement that we perceive (see) perceptual objects (composed of sense-data). In Broad's and Price's theories 'sight' includes the whole process of perception—we see physical objects—and a small terminal part of this process comprises the final link in the chain when we sense the representations cast, as it were, by these physical objects

[1] Lord Brain, 'Space and Sense-Data', this *Journal*, 1960, **11**, 177

[2] C. D. Broad, *Scientific Thought*, London, 1923

[3] H. H. Price, 'Survival and the idea of another world', *Proc. Soc. Physical Research*, 1953, **50**, 1; and his review in this *Journal* (1960, **11**, 71) of Sir Russell Brain's *The Nature of Experience*

in our consciousness—that is sense-data and collections of sense-data that make up sensible objects.

Lastly, Brain mentions the claim made by Hirst[1] that the sense-datum theory is self-refuting on the grounds that, if all that we know are our own sense-data, then we can know nothing about any physical objects that the theory alleges lurk behind these sense-data. However, Hirst[2] has more recently conceded that the sense-datum theory can legitimately base its epistemology on the claim that it gives 'the best explanation' of the world, coupled with the claim that it is fruitless to attempt to give a logical proof of the existence of the physical world.

Brain then goes on to suggest that sense-data are abstractions that

> give us information about the spatial relationships of external objects. . . . This information guides, and is validated by, our motor activities in relation to such external objects. This enables us to dispense with the idea of a multiplicity of private perceptual spaces, one for each observer (pp. 182–3).

Then he suggests that the secondary qualities of objects may be products of the activity of our nervous systems, but the primary qualities belong to the objects themselves as physical objects. However, there seem to be some difficulties about this account. Sense-data (visual sense-data for example) may certainly be considered to be abstractions in that they are arbitrarily chosen coloured patches in the visual field (see Smythies[3]). But the visual field itself could hardly be called an abstraction. It is surely a raw fact of experience. But I think that Brain would say here that a sense-datum is an abstraction from what is really given which is a *sensory experience* which may be described by the phrase 'a sense-datum being sensed (experienced, inspected, examined [illegible] by me'. He would claim that what we are really given is this 'fact of k[illegible]ed, had, etc.) b[illegible]nce or information. . . . If we start', he says, 'with knowledge or [illegible]nowledge, experi[illegible] with the subject-object relationship already given. We do not n[illegible] experience we start [illegible]ome aware of things outside ourselves because it i[illegible] with that aw[illegible]ed to ask how we be.' (p. 184). This is really the key to Brain's ne[illegible]s formulatio[illegible]areness that we begin[illegible]roposes to replace sense-data as the basic episte[illegible]w logical en[illegible] of the problem. He p[illegible]sense-data as the basic statements) with 'th[illegible]o- of knowle[illegible]ities (or statements about [illegible]ion'. His argument runs as follows: [illegible] fact

(1) P[illegible]lge, experience or informat[illegible]ies I know both where the cu[illegible] where[illegible]emise: 'When I lift a cup to my lips [illegible]p is, and

(2) De[illegible] my lips are in physical space.'

abou[illegible]duction: '. . . it follows that our sense-data may give us information

(3) [illegible]t the spatial relations of external objects.'

Conclusion: 'This enables us to dispense with the idea of a multiplicity of private perceptual spaces . . .' (p. 182).

This may be criticised on these lines:

(i) Whereas we may be well advised to base our epistemology on sensory experiences rather than sense-data, this does not give us the right to assume that our experiences are directly of 'things outside ourselves'.

(ii) Statement (3) does not follow logically from statements (1) and (2), particularly as the multiple space theory would accept as true both statements (1) and (2).

[1] R. J. Hirst, 'Perception, Science and Commonsense', *Mind*, 1951, 60, 493

[2] R. J. Hirst, *The Problems of Perception*, London, 1959

[3] J. R. Smythies, *Analysis of Perception*, London, 1956

So the conclusion of his argument—that we can now dispense with this theory—can hardly be maintained. Brain might claim, however, that he was not trying to refute the multiple space theory but was offering another and simpler one that was equally able to reconcile the apparent differences between the scientific account of perception and the common sense account that led the sense-datum theorists to construct the multiple space theories. Let us then restate and compare the two positions:

Account 1. We must start with sense-data as given (even if embedded within a sensory experience). These have spatial properties and relations. They bear causal relations to events in the brain of such a kind that they represent external physical objects. They exist in a different space to physical objects.

Account 2. We must start with (i) our awareness of external things and (ii) our given information about the spatial characteristics of things. The apparent spatial properties and relations of sense-data are really the spatial properties and relations of physical objects or their perspectives (in Russell's sense).

Account 2 is based on the premise that our awareness of objects can be equated with our information about them. As Brain says '. . . being aware of objects is identical with receiving information about them', . . . (p. 183). 'Awareness' is at the best of times an unruly word. It is used in one technical sense (knowledge by acquaintance) as the unanalysable process whereby we experience our own sensations. It is also used in common speech to mean 'knowledge of fact' e.g. 'I am aware that it is raining'; that is knowledge by description. Much confusion is caused by the fact that both these meanings can be applied to our relation to our own sensations. One cannot accept as a self-evident fact the identification of being aware of objects with receiving information about them. Suppose that I look at an object, say a leaf. What information do I obtain just by looking at it? Surely any information is gained only when I make specific observations on the leaf, as to its size, shape, colour, relation o other leaves etc. There is a cognitive element in gaining information about the af which is something over and above the mere presence of the 'leaf' in my sense-ld at any particular moment. But even if we admit that in some sense to be aware bjects is to receive information, even if only potential information, are they arily identical? Sense-data are particular existents, even if they are really parts ger whole—the total sensory field—or again parts of the total context of some-g aware of this total field. But information is itself a highly abstract concept. It can be defined as follows: 'A piece of information is a statement that something is, or is not the case.' But the term sense-datum cannot properly be defined in words at all. It can only be defined ostensively. This would argue that of the two 'information' was more abstract than 'sense-datum', and thus less suitable to act as our basic epistemological coinage. Furthermore the description 'information about sense-data' is meaningful and such information is frequently given out by philosophers such as Price and psychologists such as Klüver. Can Brain distinguish then between information about objects and information about sense-data, if the former is identical with being aware of objects? For he says '. . . our sense-data may give us information about the spatial relations of external objects' (p. 182), but he leaves himself no room to include in his system information about sense-data. He assumes that the information that we are *given* is information about external objects and thus he feels that we can do without the idea of a multiplicity of private perceptual spaces. However, we are also *given* information *about* sense-data, using equally valid means of

gathering information—Quinton's introspective analysis—and the most obvious item of information about sense-data is that they are spatial. And this entails that we are not entitled to dispense with the idea of a multiplicity of private sensible spaces.

(iii) Brain's theory suffers from the difficulty inherent in Locke's original ancestor of his theory. That is that it is difficult to see how the colour and shape of our sense-data, which appear so insolubly joined, have such disparate origins—products of our nervous system in the first case and a direct prehension of physical objects in the second.

(iv) Brain's return to direct realism brings in its train the familiar difficulties of the theory—in particular the time-lag between physical events and our perception of these events. It is true that this only leads to any practical difficulties when we consider astronomical events. His solution to this is to say that '. . . the sun, as the object of my perception of light and heat, is equivalent to the light and heat waves during the last part of their passage through the atmosphere, . . .' (p. 184). Brain does not specify precisely which part of the atmosphere is involved, although his formulation leaves him vulnerable to such questions to which it would appear that no sensible answers could be given. Our perception of the sun is analysed further into the facts (i) that our information about the sun, as an object, turns out to be accurate information about its past state, and (ii) that we are thereby enabled to make the correct motor response towards such objects, e.g. shading our eyes from the sun. Such statements are certainly true but they do not analyse the situation fully. For he also specifically states that '. . . the astronomer's sun which is by now in a slightly different position from the sun *which I see*' (my italics, p. 184). The sun which I see (and feel) can hardly *be* a packet of light (and heat) waves travelling through the atmosphere; for the meaning of the phrase 'I see the sun' is quite different from the meaning of the phrase 'I see a packet of light waves travelling through our atmosphere'. The saving statement that light waves carry information from the sun to us is so general that it is satisfied by almost any theory of perception including the representativ theory and cannot thus be used as the basis to abandon any such theory. Ther fore we can hardly '. . . interpret object as meaning the last phase of the energy tl propagate before it reaches our bodies, . . .' (p. 186). Brain offers us another c however, '. . . or we must say that the spatial characteristics of which we present knowledge are those of their [astronomical object's] past, and often th remote past' (p. 186). Here the object still eludes our grasp for this rea universe of objects that we can see to a collection of the past states of objects. It would seem that the past states of objects cannot be said to exist in any simple sense. For if we say that we only know objects through their past states, or only see them in the past, this necessarily implies that they have a present state which we do not see. The theory of direct realism cannot allow that objects have such present states. Otherwise we cannot be said to have any direct prehension of *the* object. Yet I suppose that every astronomer looking at distant galaxies knows that he is seeing the galaxy *as it was* 10,000,000 years ago and that this same galaxy *now* is out there in space and *will be seen* by men on earth 10,000,000 years hence. I have analysed elsewhere[1] the further paradoxes into which the theory of direct realism as applied to astronomical events leads us into. It is a great advantage of the representative theory, particularly in its modern form of the multiple space sense datum theory, that it

[1] J. R. Smythies, loc. cit. (appendix 2)

avoids these difficulties since it does not confuse our sensations of objects with the objects themselves. An objection that has been brought against the multiple space theory has been that it is ridden with complex and difficult problems about the relations between the various spaces. This accusation could hardly be levelled against Price's version of the theory which recognises the simplest possible relation between the spaces, that is : *no* spatial relation merely causal ones. This elegant simplicity should be set against the appalling tangles into which the theory of direct realism gets us when we consider our perception of distant objects.

Thus in conclusion we can suggest that Brain's arguments are not logically relevant to the problem of the status of sensible space, that is the status of sense-data and their spatial properties and relations. It is certainly true that sense-data convey information about physical objects. Yet, such is the logic of non-symbolical representation, this is perfectly compatible with the fact that sense-data also purvey information about themselves. The main such item of information is that they constitute a spatial system. Furthermore the fact that sense-data convey information about physical objects in no way precludes the possibility that they bear *other relations* to physical objects—for example spatial relations and causal relations. Brain's main error is to assume the contrary.

J. R. SMYTHIES

REPLY TO PROFESSOR SMYTHIES

IN my paper ' Space and Sense-data ' I sought to show that it is unnecessary to suppose that sense-data, or sensible objects, occupy a space, called sensible or perceptual space, which is different from physical space. I shall not comment in detail on Smythies's of ', since I find myself in complete agreement with his ' conclusion ', though neces- hat my ' main error is to assume the contrary '. I agree that (1) ' sense-data of a lar- ormation about physical objects '; (2) ' sense-data also purvey information one bein- emselves ' (though I should express this somewhat differently); (3) one ' item of such information is that they constitute a spatial system '; and (4) ' the fact that sense-data convey information about physical objects in no way precludes the possibility that they bear other relations to physical objects—for example spatial relations and causal relations '. Where I differ from Smythies is in not believing that any of these statements implies that ' perceptual space is a different space to physical space '.

Since Smythies refers me to Price and Broad, instead of devoting time to discussing his comments on my views, it seems to me more useful to consider the validity of the arguments which have led those who advocate some form of ' multiple space sense-datum theory ' to adopt a view which is contrary to everyday experience.

Briefly, Smythies's[1] argument is that sense-data, such as those in the visual field, exhibit spatial relationships, but these spatial relationships cannot be identical with the spatial relationships of the events in the nervous system which underlie them, nor with those of the more remote physical events which excite the activity of the nervous

[1] J. R. Smythies, *An Analysis of Perception*, London, 1956

system. This, in his view, refutes the hypothesis of psychoneutral identity, and also involves the postulation of different spaces for sense-data and physical objects, of which the brain is one. Price[1] also claims that ' visual images . . . have extension and shape, and they have spatial relations to one another. But they have no spatial relations to objects in the physical world. If I dream of a tiger, my tiger-image has extension and shape . . . but suppose we have to ask how far it is from the foot of my bed, whether it is three inches long, or longer, or shorter: it is not obvious that these questions are absurd ones? . . . In the space of the physical world these images are nowhere at all, but in relation to other images of mine, each of them is somewhere. Each of them is extended, and its parts are in spatial relations to one another. There is no *a priori* reason why all extended entities must be in physical space '. Thus both Smythies and Price assume that, because sense-data have spatial relations with one another, there must exist ' a space ' in which they have their being, and since it is not at first sight obvious how the spatial relations of sense-data are to be correlated with those of events of the physical world, it is further assumed that the correlation of ' the space ' of sense-data with the space of the physical world constitutes a problem. It may be suggested in passing that one reason why Price finds it difficult to say how far the tiger in his dream is from the foot of his bed is that he is not aware of them both at the same time. When he is dreaming of the tiger he does not know he is in bed, and, when he wakes up, there is only the memory of the tiger-image. But it seems equally difficult to relate ' the space ' of one dream with that of another. If I dream first, like Price, of a tiger under a palm tree, and then of a polar bear on an iceberg, how am I to ascertain how far it is from the tiger to the polar bear? Do they exist in the same ' dream-space ', or must we postulate two different spaces, one for each dream? If such questions ' have no meaning ', may it not be because it is a mistake to suppose that, because we can describe images as having spatial relations, we must postulate a private space for their existence? Images in a mirror have spatial relations with one another in three dimensions, and appear in positions in physical space behind the mirror, and both Broad[2] and Price[3] use a complex terminology to describe them.

Suppose I am looking at a picture of a house and garden with a man standing in the garden. If I ask, what is the height of the house, there are two possible answers. If I measure it on the canvas, the answer is, let us say, five inches, but if I assume the man to be of average height it is equally true to say that the house is about thirty feet high and the same distance away from him. If, moreover, I measure on the canvas the heights of the house, and the man, and also the distance between him and the house, if he is ' nearer to me ' I shall find that the ratio of his height to that of the house is greater, and the ratio of the distance between them to the height of the house is less, than in the physical world, which the picture represents. Here, then, it might be said that we are dealing with two ' spaces ', and that the relative size of three objects in one ' space ' is different from their relative sizes in the other ' space '. Moreover one ' space ', that of the surface of the canvas, is two dimensional, while the other ' space ', that of the scene represented, is three-dimensional. Nevertheless it is not meaningless to ask how the space of the images in the picture is related to the space

[1] H. H. Price, *Proceedings of the Society for Psychical Research*, 1953, 50, 1
[2] C. D. Broad, *Scientific Thought*, 1927
[3] H. H. Price, *Perception*, 1942

special case of *T*. Hence, contrary to Popper's claim, Carnap is not committed to (4) on the ground that it is derivable from *T*.

Indeed, it should be noted that, as Carnap understands '$Co(x, z)$', (4) is ruled out by the probability requirements which, he maintains, a confirmation function must satisfy. For (4), as Carnap understands it, is equivalent to

If $c(x, z) \leq c(x)$, and $c(y, z) > c(y)$, then *not*-$[c(x, z) \geq c(y, z)]$.

But this is violated, for example, by the following values, which are certainly permitted by the axioms of probability:

$$c(x, z) = 3/10 \qquad c(y, z) = 2/10$$
$$c(x) = 5/10 \qquad c(y) = 1/10.$$

Thus, by proving that Carnap must accept (4), Popper would have uncovered an even more striking contradiction in Carnap's scheme.[1] However, since Popper's argument does not succeed in showing that Carnap must be committed to (4), it fails to unmask any inconsistency in Carnap's system. *A fortiori*, the argument in question does not succeed in demonstrating the necessity for Carnap to abandon the Identity Hypothesis.

PETER ACHINSTEIN

[1] Popper, of course, has recognised this fact. See, e.g. *The Logic of Scientific Discovery*, p. 390.

REVIEWS

The Ethical Animal. By C. H. Waddington.
George Allen and Unwin, London, 1960. Pp. 230. 25s.

THE problems of philosophy may be perennial, yet some of them press harder on some generations than on others. The fact that judgments of value differ from man to man and from culture to culture was noticed by Herodotus, and was certainly very plain to Plato. For us this diversity, especially at the intercultural level, raises the most urgent philosophical problem of our day. Can we evaluate systems of evaluation, can we grade cultures, and if so, by what right?

It is this problem to which Professor Waddington's argument is directed; if he had solved it, this book—or his earlier *Science and Ethics*, of which the present book forms in a sense a revision—would have constituted a major philosophical achievement.

What, and how successful, is Waddington's answer? 'It is the thesis of this book', he writes, 'that the framework within which one can carry on a rational discussion of different systems of ethics, and make comparisons of their various merits and demerits, is to be found in a consideration of animal and human evolution.' His solution differs, however, he declares, from that of other evolutionists. Julian Huxley's evolutionary ethics, he rightly observes, is viciously circular: by our ordinary ethical standards, we judge evolution to be good, and then we justify those same standards by reference to evolution. Simpson and Dobzhansky, on the other hand, lay too much stress, for his liking, on the factor of freedom in the sense of arbitrary choice. His solution, he argues, is non-circular, at least in any vicious sense, because it is based on a criterion over and above the source of our individual ethical judgments, a criterion which he calls 'biological wisdom'. Moreover, it is rational, since it is grounded, not in an arbitrary choice, but in 'an induction from the properties of individual ethical systems'. Biological wisdom, in other words, claims to be, not a super-ethic, but a scientific generalisation enabling us to judge between the diverse ethical systems which the ethicising animal, man, has developed in the course of his evolution.

What is this biological wisdom? Although Waddington's argument is often obscure and confusing, I think I may hazard the statement that biological wisdom is said to involve four things. Firstly, the biologically wise man recognises the fact that human evolution has its own peculiar, *socio-genetic* mechanism: in brief, the transmission of culture through authority

and learning from one generation to the next. It is the function of 'ethicising' to mediate this process. Secondly, the wise man takes account of the difference between *cladogenesis* and *anagenesis* in evolution. Within a given organisational type we have, in cladogenesis, according to Waddington (after Huxley, after Rensch), a spread of diversification on a given level of evolutionary development. In anagenesis we have, still within the given type, a forward movement from a less to a better developed version of this type. Examples: *cladogenesis:* the proliferation of browsers in the equid genealogy, even during the period when the development of grazing types had begun; *anagenesis:* the advance along the line of grazing animals now perfected in the modern horse. Of such 'forward' movements, in turn, those are peculiarly significant which are 'open-ended', i.e. which lead not to evolutionary dead-ends but to possible avenues of further advance. This distinction leads the wise man, I can only say, to do one or both of two things, for the text here is elusive and ambiguous. Either (thirdly), he singles out the one line of anagenesis characteristic of evolution in general: this would be an overall direction of evolutionary advance, from type to type, rather than anagenesis as originally defined, which is restricted to progress within a given type. It would be, in effect, an anagenesis of anageneses which the biologically wise man approves and seeks to promote. What this trend is or might be, however, is again obscure. In one formulation suggested by Waddington, for example, it is 'the increasing ability to utilise for the maintenance of life more and more complex relations between environmental variables'. Here it seems to be a question of 'better' techniques for survival, the maintenance of life being the common end. In other passages, however, Waddington speaks of an overall tendency in evolution toward a 'richness,' a 'depth', a 'fullness' of life: now not life itself, but in some way or other a *good* life is what evolution is said to have produced. Or, on the other hand, fourthly, one infers from some of Waddington's statements that it is a possible open-ended anagenesis, not in evolution as a whole, but simply within our own organisational type that the biologically wise man apprehends and appreciates. Thus in one passage, for instance, it is said to be the development of the super-ego, characteristically produced by our socio-genetic evolutionary system, which is to be fostered. (The dogmas of psychoanalysis, I may say in passing, are accepted by Waddington with a good deal of naiveté as established truths of science. Many psychiatrists, let alone philosophers, would be inclined to question his confident acceptance of authority here.) Elsewhere, on the other hand, he writes in more general terms: 'our argument leads to the conclusion that biological wisdom consists in the encouragement of the forward progress (anagenesis) both of the mechanism of the socio-genetic evolutionary system, and of the changes in the grade of human organisation which that system brings about.' This is a summary clearly

in terms of point four; yet I suspect that it would not be possible were not the more general conception of evolutionary advance lurking somewhere offstage.

I hope this is not an unfair summary of this extraordinarily cloudy argument. Some of the difficulties of restating it may emerge from the following criticisms.

Let me say first that I heartily agree with Professor Waddington when he regrets the narrowness of much academic philosophy, and the failure of philosophers to bring relevant material from psychology and biology to bear on their reflections. Even here, however, he has misinterpreted many of the examples he cites against us. When, to take just one case, Professor Emmet tells us that the concept of 'evolutionary progress' may be one in which we are reading back into the natural world concepts derived 'from analogies with human actions and purposes,' Professor Waddington comments: 'She is basing her thought on the implicit assumption that human actions and purposes could be something completely external to, and independent of the natural world.' But it seems to me quite obvious that exactly the contrary is the case. Professor Emmet is not arguing that our purposes are ever wholly independent of nature, but that nature is sometimes independent of our purposes—and 'progress' being a distinctively human and purposive concept, we have no right to impose it without further guarantee upon that nature of which we are, *qua* nature, a small and insignificant part.

'Progress,' indeed, is one of the concepts which appears in just such an uncritical use in this very book. What is evolutionary 'advance' or 'improvement'? Is it purely improved techniques for survival that are meant? If so, there are two points to be made. First, in terms of survival value, in terms of a strict selectionist theory of evolution, we have no right to speak of progress as between one kind of organism and another. As both Darwin and T. H. Huxley recognised, there is, or ought to be, no distinction whatsoever between higher and lower in evolution so conceived. The insects have survived and so have we, and they have survived a good deal longer and are a good deal more numerous than we are. Secondly, however, if, as Waddington suggests, some sort of more complex organism-environment relation somehow 'objectively' marks an 'advance', and if we have in fact achieved such a relation, then nevertheless, if it is survival techniques we are judging, and if biological wisdom is to lead us to choose in the light of such a criterion between one ethicising system and another, the system which will lead to 'the maintenance of life'—any life, if only life—will be the one to choose. Now that may well be, for example, the system which practices the most efficient brainwashing: a procedure which Professor Waddington appears to deplore. Yet on 'scientific' grounds, and in terms of survival value alone, I see no foundation whatever for his deprecation of it.

It is precisely in terms of our *ethics*, not of any scientific generalisation, that we deplore it. ' The maintenance of life ', however, is only one of Waddington's multifarious formulations of the chief line of evolutionary advance. If, on the other hand, it is not life, but richness and depth of life in terms of which we are to make our choice, if it is an ' active, creative life ' we are after, then this is indeed progress in the light of our own values, our ideals, which we are, as Professor Emmet warned us not to do, reading back into nature through the accordion-like use of that expansive concept ' evolutionary progress '. This, again, is no scientific generalisation, but the statement of an ethical appraisal.

Again, a similar ambiguity affects Waddington's use of ' anagenesis ': as I have already pointed out, this concept is introduced as applying within a single type of organisation, but is somehow, without warning, extended to refer to an advance *between* types, an advance in evolution as a whole. Yet how can we, who are *within* one type, judge ' objectively ' such overall advance?

Yet again, for example, we are told that we can see more in evolution, for our purposes, if we look at it ' organismically ' rather than purely mechanistically. Evolution in itself includes no values, no ' harmonies ' or ' wholes ', and ' organicism ' is simply read into the evolutionary story by the all-too-human assessor of it for the purpose of assuaging his human needs. In that case, however, the levels of organisation Waddington continues to talk about have no *scientific* meaning, but are pure ' as-ifs ' used to soothe our ethical feelings, and references, for instance, to ' the clear-cut hierarchy of organic forms recognised since Aristotle ' give us no information whatsoever about nature, let alone a ' scientific ' yardstick by which to judge between the merits of divergent forms of life or systems of authority.

I could go on listing such ambiguities and inconsistencies, all of which stem in one way or another from a failure to distinguish between philosophical (in particular moral) problems and causal analysis, between the desir*able* and the desired, between ought and is. One passage will perhaps illustrate this fundamental confusion at least for philosophical readers. In discussing criticisms of his earlier book, Waddington quotes the following statement by Professor D. D. Raphael:

> Waddington thinks the answer to my question: ' What am I to do and for what reasons? ' is the same as the answer to the question ' What will I do and for what causes? ' This is why he thinks that a causal account of how ethical judgements have come to be what they are can supply a criterion or rational ground for the ethical judgments we should make. But since the two questions and the kinds of answer they seek are of different logical types, Waddington's argument for using the direction of evolution as the direction for ethical judgments rests on a logical confusion.

In answer, Waddington now replies, ' Here I take it that " reasons " are

verbal arguments deducible from some theory which accounts for certain phenomena in terms of the causal properties of the components of the system in which the phenomena occur'. It seems to me superfluous to comment on this extraordinary statement. But, Waddington continues, this is not the main point: more important is that Raphael's second assertion is still in the first person, whereas his 'wisdom' refers to some 'larger entity, such as the human species as a whole, or even the living world as a whole.' He reformulates Raphael's statement, therefore, to conform to his own belief:

> Waddington thinks the answer to the question 'What would it be wise for me to do and for what reasons?' can be deduced from the answer to the question: 'What has the world at large been doing during its history and from what causes?' This is why he thinks that a causal account of how individual ethical judgments have come to be what they are can supply a criterion or rational ground for the judgment between different ethical beliefs which it would be wise for us to make. These are two questions and the kinds of answer they seek are of different logical types. Waddington's argument for using the direction of evolution as the criterion for judgment between ethical beliefs rests on acceptance of this.

Without denying that the whole of history may have a bearing on what it would be wise for me to do, I may, I hope, simply assent wholeheartedly to Raphael's criticism, which is clearly valid against this book as against the earlier one, and suggest, in conclusion, a further revision:

> Waddington thinks the answer to the question 'What would it be wise for me to do and for what reasons?' can be deduced from the answer to the question 'What have living beings been doing since the Cambrian and from what causes?' ... since the two questions and the kinds of answer they seek are of different logical types, and therefore, though each has relevance for the other, neither is deducible from the other, the one referring for its answer primarily to my ultimate ideal of what life ought to be, the other primarily to a factual account of what life is, Waddington's argument for using the direction of evolution as the direction for ethical judgments rests on a logical confusion.

Criteria of objectivity, of relevance, of accuracy, are indeed necessary to ascertain what goes on; but what happens cannot, apart from our evaluative assessment of it, *create* criteria in its turn. If *oughts* can be deduced from *is*'s, it is only from the *is* of an *evaluation* (like the utilitarians' 'all men value pleasure,' etc.) that they can be derived. Only by smuggling 'progress,' 'richness,' 'criterion,' 'rational' into nature, can we pretend to draw such qualities out of it again. This is a fair, a necessary procedure if we know what we are doing, but a snare and a delusion if we think that the practice of 'normal scientific methods' and not the articulation of our deepest hopes and most personal aspirations is what we are about.

MARJORIE GRENE

The Natural Philosophy of Time. By G. J. Whitrow.
Thomas Nelson and Sons Ltd, London and Edinburgh, 1961. Pp. xii+324. 42s.

DR WHITROW is Reader in Applied Mathematics in the University of London at the Imperial College of Science and Technology. He is the author of *The Structure of the Universe* (1949), revised under the title *The Structure and Evolution of the Universe* (1959), and of a considerable number of papers on the philosophy of science and various aspects of relativity. The book now under review has six chapters: Universal Time, Individual Time, Mathematical Time, Relativistic Time, Space-Time and Cosmic Time, The Nature of Time. The style is clear and imaginative. Mathematical symbolism is introduced where required, but the greater part of the book is open to the non-mathematical reader. In the text, and particularly in the footnotes, there is a wealth of information culled from many sources, ancient and modern. There is an excellent index. The volume is well and carefully produced—I noticed only a few trivial misprints.

Having thus discharged the prime duty of a reviewer (describe the book), I now feel free to look around a bit, perhaps to pat the author on the back for giving us a most scholarly book on a most interesting and important subject, and perhaps to pick a few quarrels with him.

Dr Whitrow is a polymath, but definitely, and that is what gives charm and richness to his book. After the Contents, he salutes the reader with seven quotations ranging from St Augustine through Lewis Carroll and Samuel Butler to Whitehead, including a very apposite one from C. D. Broad: 'Our knowledge of time as of space owes more to the labours of mathematicians and physicists than to those of professed philosophers.' Yet it seems to me that in this book it is the philosophical strand which dominates. Nevertheless the balance is well maintained, and we see the author in turn as physicist, historian, biologist, psychologist, mathematician, relativist, and cosmologist. The range of his reading is enormous. I cannot resist the mischievous temptation to censure his omission of reference to the supply of candles of equal length used by the great King Alfred (cf. M. Bloch, *Feudal Society*, p. 73).

Dr Whitrow combines with his erudition a refined sense of diplomacy. The semantic undertones of his chosen title are calculated to seduce both the philosopher (natural? well, not unnatural surely) and the physicist who recalls dimly that certain venerable chairs of mathematical physics carry the name 'natural philosophy'. So Dr Whitrow gallops into the ring with one foot on the saddle of Rozinante and the other on the back of Sancho's honest and reliable ass. By changing from one mount to the other, he is ready to tilt against windmills or to jog along the quiet ways of scientific common sense.

Windmills ! If you wish to live for ever in the mouths of men, do not waste your time building a castle—windmills are much more permanent. But, to call a truce to allegory (as if we ever could), these windmills are *paradoxes*. I do not think that Dr Whitrow creates any himself, but he loves to play with those that are available. The paradoxes of Zeno occupy pages 135-152 and the Clock paradox pages 215-222. It takes only one page (p. 149) to deal (unsympathetically) with the Tristram Shandy paradox of Bertrand Russell. To me, the greatest paradox of all is the intellectual joy men find in paradoxes—I could understand it, perhaps, if the word *paradox* were used in its old and literal sense (contrary to received opinion or expectation). I have never held with the Gospel according to St John, Chapter I, verse 1—for me ideas come first, and words lag behind a long way.

How pleasant, when saddle-sore from Rozinante's bony back, to change to Sancho's ass and explore with Dr Whitrow, in his all-too-brief account (pp. 59-71), biological time as exemplified in bird navigation, the expansion and contraction in the pigment cells of the common fiddler crab, clocks in unicellular algae, and the rhythms of the electro-encephalogram, in the midst of which J. Locke unexpectedly raises his head, only to be put back in his place—his belief ' obviously quite inadequate '.

But my space is limited, and I must get to the only part of the book with regard to which I might be expected to display professional competence. When I read, with some amazement, my own name standing there in the second line of the Preface, I had high hopes. These were damped a little when I read in the next paragraph that the first draft of Dr Whitrow's book was already completed when my article appeared in *The New Scientist*. But I still had a hope, because, after all, a first draft is only a first draft. It can easily be destroyed. The last candle of hope expired when I went on to read in the third paragraph that E. A. Milne had been preaching my message a quarter of a century ago.

To clear up this deplorable situation, I must fall back on an analogy—the ' noise ' and the ' information ' that come to us over a telephone line. If you lift the receiver and listen to nature, you will probably hear nothing but noise. Exasperated, you ask switchboard to connect you with yourself. This is much more satisfactory, for you can talk to yourself, and, even if noise is not entirely absent, the information travels from you to yourself, and perhaps to a circle of listeners-in. But there are those who are not satisfied by this. They go on listening to nature and (if they possess ears of a type denied to most of us) information begins to take shape. The message may come in garbled form. Then it must be decoded and reformulated again and again until those elements of noise which have got mixed up with the message have been eliminated. When this has been done, you can get back on your closed circuit and tell yourself all about it over and over again.

This may seem to be very much as it was before, but actually there is all the difference in the world. Previously, you had to talk out of your head with no real information to impart; now you may claim that you are repeating the word of God, purged of that noise which seems always to accompany revelation.

Where do we all fit into this picture ? Einstein the listener, Minkowski the decoder, Einstein again the listener with Minkowski's code at his elbow, and finally us on our closed circuit saying it all over and over again. And me in particular ? Well, I hate noise, and I am trying to make the message of Einstein and Minkowski come over a little clearer. I add nothing essential, only subtract the inessential. Have I, in attempting this, unwittingly followed Milne ? The rudeness of the marginalia pencilled twenty-five years ago in my copy of his *Relativity, Gravitation and World-Structure* would seem to belie this. And I find myself as early as 1921 putting forward in a letter to *Nature* the essential part of the idea in question, viz. the reduction of all measurements to chronometry. I was never able to swallow Milne's equivalent observers; they make nice conversation for the closed circuit—a conversation lasting 360 pages—but then I can think of much more interesting things to talk about.

The last three chapters of Dr Whitrow's book give an excellent account of the ways in which men have thought about time since the coming of relativity, together with the statistical theory of time (thermodynamic irreversibility). Whether the ways in which they *have* thought are the ways in which they *ought* to think, that is a different question, a rather personal question, since it involves individual modes of thought. Therefore I restrain my missionary zeal, and refrain from telling Dr Whitrow how he ought to have written those last three chapters in order to let relativistic information about time ring out clearly above the rattle of the dead bones of the past.

Finally, two points of detail. On pages 301-302 there is a discussion about the relative speeds of particles, or pseudo-particles, some of which may be moving ' faster than light '. The argument is set in Minkowskian space-time, and we know that two unit 4-vectors define just one invariant, viz. their scalar product. The game is to dress up that scalar product in some way which will not shock the Newtonian (how those dead bones rattle!). To do this effectively, we need, not two particles, but a particle and a frame of reference, the latter being an orthonormal tetrad having (as an orthonormal tetrad must) a timelike 4-vector and a spacelike 3-flat orthogonal to it. The 4-velocity of the particle is resolved by orthogonal projection on the tetrad, and the components of the 3-velocity of the particle are given by the quotients of the spacelike components of the 4-velocity by its timelike component. This procedure works whether the speed of the particle is less than, or greater than, the speed of light. But if we start with

two particles both travelling faster than light, we cannot attach to either of them a suitable tetrad to act as frame of reference. If the ex-Newtonian insists that, even though they be moving faster than light, a particle must have *some* speed relative to another, I would offer him the Minkowskian scalar product aforesaid to play with, and, if he refuses it, I would tell him politely to go and rattle his bones somewhere else.

The other point is a small one. A positron and an electron cannot change into a single photon, or vice versa, with conservation of 4-momentum. This is a simple theorem in Minkowskian geometry, and the first footnote on page 304 is false. The infection spreads back to page 281. But the case is not serious enough to call for an operation—all we need is a second photon or a whiff of that virtual anaesthetic with which quantum physicists deaden intellectual pain.

J. L. SYNGE

The Atomic Problem: a Challenge to Physicists and Mathematicians. By Lancelot Law Whyte.
George Allen & Unwin, 1961. Pp. 56. 7s. 6d.

IN this short, tersely-written essay, the author first reviews the history of the basic unsolved problems of atomic physics. But his main purpose is to invite all whom it may concern to take note of the present impasse in theoretical atomic physics, and to challenge them to seek the unifying theory which, the author frankly admits, he has himself sought in vain for many years. The book is therefore addressed to applied mathematicians and theoretical physicists; it makes no concessions to the general reader.

That there is an impasse and that there is a need for unification of 'all types of particles and fields, including their interactions and combinations', is unlikely to be disputed. What is less likely to win unanimous assent is the author's programme for attaining the desired objectives. Any theoretician working on the problems of atomic physics foresees future theoretical developments as direct extrapolations of his own line of work. And though he disclaims that his is a personal programme, Mr Whyte is an individualist who has long ploughed a lone furrow and his projected programme naturally reflects his own unique experience and special interests.

The immediate problems he poses for solution, not surprisingly to those aware of Mr Whyte's interests, are concerned with the analysis of chiral deformations and relaxations of point sets. He argues that solution of the main problem depends on identification of 'all the invariants of current theory, and their interactions, as special components of variants' which he regards as 'chiral, centred, one-way processes in complex systems of primary particles'. These primary particles are specified as a class of permanent

nucleons possessing a basic unit of mass which cannot be converted into radiant energy. And it is these nucleons which appear to offer the only possible frame of reference since all fields and interactions can be regarded as occurring between such nucleons or systems of such nucleons. The series of unstable ' elementary particles ' discovered since 1932, he dismisses as ' spurious ', i.e. as mistaken projections of atomic concepts into the high-energy processes which occur in the micro-structure of matter.

He suggests that it is necessary, though psychologically difficult, to discard time invariants from the basic theory, i.e. that the concepts of one-way processes derived from statistical mechanics be extended to micro-structures. He is very interested in the dimensionless numbers; the first aim of his unifying theory is ' to explain how the numbers 1/137.03(7) and 1/1836.12 . . . come to be the primary quantising operators of atomic physics '. The approach suggested is thus, in some respects, reminiscent of Eddington's *Fundamental Theory*, though Eddington receives no more than passing mention.

The author stands in a strong logical position. His arguments can be disputed, but they cannot be refuted except by a successful unifying theory based on principles different from his own. Therein lies the challenge he throws down. There is a faint suggestion about the book that the author is now retiring from the active struggle and that this essay is his scientific last will and testament. But I hope not, because no-one is likely to be better qualified to pursue Mr Whyte's programme than Mr Whyte himself.

B. C. BROOKES

Semantic Analysis. By Paul Ziff.
Cornell University Press, Ithaca ; Oxford University Press, London, 1959. Pp. x+247. 40s.

ON page 1, the author announces his aim, which is ' to answer the question, " What does the word ' good ' mean in English?" '. On the last line of the last page, there it is: ' . . . this is what " good " means: answering to certain interests.' But the author has to worry about the confirmation of this answer. (The book was written backwards, apparently, like Wagner's *Ring*.) Ziff is forced to deal with the general question: How does one find out what the Trobriand Islanders (or well-bred New Englanders) mean by the words they use. This question belongs in an area which elsewhere I have called ' theoretical descriptive semantics ' to distinguish it from pure formal semantics (of, say, Carnap). Ziff approaches the whole matter from descriptive linguistics rather than from formal semantics.

In arriving at a dictionary entry (this is what Ziff aims at in the case of ' good ') we take a corpus of utterances, subject them to certain procedures described in some detail in the middle chapters and finally arrive at a

'meaning' of the word in question. The procedure is partly semi-formal: principles are stated and appealed to; and partly intuitive: some utterances are expelled from the corpus because they 'sound odd'. In the last chapter Ziff examines about one hundred and fifty characteristic utterances containing the word 'good'. (We say 'a good cat', 'a good cadaver', but not 'a good gila monster' or 'a good corpse'.) He concludes: 'good' means *answering to certain interests*.

This may or may not be good lexicography, but it will strike a philosopher as *obscurum per obscurius*, since nothing very much is said about 'interest', except to differentiate *interest* from *need, desire, passionate concern*, and the like. (The point would be that '"good" has a relatively dispassionate feeling' (p. 222), a certain air of judiciousness about it.)

Judgment of the book is rendered difficult by reason of its intensely irritating style: (*a*) A Wittgensteinian format of numbered short sections in some cases conceals connections, in others serves to conceal a lack of connection. (*b*) There are periodic bursts of misplaced belligerence (e.g., '"Hello!" has no referent', plus its context on p. 82), including what one takes to be some badly aimed pot-shots at Quine, who isn't even mentioned by name. Also, the middle chapters abound with some astonishingly bad arguments for unplausible or pointless quasi-philosophical (i.e., semantical) theses.[1] (On p. 86, e.g. the example is offered of a person who knows not one word of Chinese but is prepared to talk about Hsieh Ho. It is inferred that 'Hsieh' and 'Ho' and generally any proper names are not words. To which it would be replied that the same line of argument would apparently show that 'kindergarten', 'soprano', and 'bazaar' are not words. The argument must therefore be fallacious. In any case, what difference does it make whether proper names are classed as words or not?)

Oddly enough, all the dubious stuff could be dropped and the book would remain essentially intact. Even though it is to be regarded only as 'an informal introduction to and sketch of a rigorous semantic theory' (p. 198) it has the merit of showing clearly that the business of arriving at dictionary definitions by using the we-say-we-don't-say method—if the game is to be played seriously and for its own sake—is much more complicated than one might realise.

N. L. WILSON

The Physicist's Conception of Nature. By Werner Heisenberg. Hutchinson, London, 1958. Pp. 192. 16s.

ALTHOUGH not an anthology, this small book is disjointed. The first two chapters present a lucid statement of contemporary methodology, its

[1] Lest this seem irresponsible I have prepared a fifteen page mimeographed analysis of some of the author's contentions, and anybody can have a copy by writing to me for it.

motivation and development, against a background of modern atomic physics. The third chapter, somewhat autobiographical, emphasises the value of a classical education. It is most interesting as a clue to Heisenberg himself. The tone is occasionally heavy, great stress being placed upon the importance of 'faith' and 'spirit'. The last three chapters present interesting excerpts from Kepler, Galileo, De la Mettrie, De Broglie, and several others, chronologically arranged and supported by a brief running commentary. Heisenberg is attempting here to outline 'the development of modern science' by quoting some of its 'most prominent exponents'. This section is notable for the stress placed upon the attitude of the various scientists.

B. DUNHAM

The Hollow Universe. By Charles de Koninck.
Oxford University Press, London, 1960. Pp. xi + 127. 12s. 6d.

THE Whidden Lectures given at the University of Toronto aim at 'helping students to cross barriers among the academic departments of a modern university'. Professor de Koninck gave the fourth in the series in 1959. His lectures were an account of what he regards as the hollowness of the universe which appears from the recent developments of mathematics, physics, and biology. In essence he points out that scientific abstraction, involving as it does ontological abstraction, although successful in prediction does leave out much that people might wish to say about the Universe, and indeed much that perhaps should be said.

I would think that few philosophers of science would disagree with him. After all this is the very nature of abstraction. His accounts of the processes involved and their consequences are, I feel, rhetorically biased, and need not be taken too seriously. They are in places somewhat irritating, because Professor de Koninck is putting them up as Aunt Sallies. However, for the scientist who has not faced up to the philosophical consequences of much of his own methodology they are extremely stimulating.

If science has progressed by asking 'How' questions, then scientists *qua* scientists must be sure that they do not want to ask 'Why' or 'What is the nature of . . .' questions. As citizens they may well need to ask the latter sort of question. In this case Professor de Koninck should have considered in more detail the methodology by which they can obtain satisfying (as distinct from satisfactory) answers.

I recommend this book to those who want a stimulating and argumentative account of the consequences of abstraction in modern scientific methodology.

R. F. J. WITHERS

The Development of Physical Theory in the Middle Ages. By James A. Weisheipl.
Sheed & Ward, London, 1960. Pp. 92. 4s.

THIS sketch is fairly well written in its *genre*, but it is very definitely an introductory pamphlet, and most of the space is taken up with a general account of the principles of Aristotelian philosophy. The only parts of physical theory in a strict sense which are mentioned are Buridan's theory of *impetus* (momentum) and the Merton rule according to which a body moving with constant acceleration may be taken to go through as much space in a given time as it would have traversed at its mean velocity if that had been maintained constant for the time. But these are both well-known from the histories of science which the author lists at the end of his work.

WILLIAM KNEALE

English Philosophy Since 1900. By G. J. Warnock.
Oxford University Press, London, 1958. Pp. x+180. 7s. 6d.

THIS book touches upon various highlights of recent English philosophy. It is modest in initial claims, but not in subsequent tone. The author regards himself as a 'professional', and deplores unprofessional intrusion into the practice of philosophy. The 'good' and 'bad' features of various writer such as Moore, Russell, Wittgenstein, and Ryle are explained to the reader in rather simple terms. In discussing a position with which the author disagrees, he often resorts to terms such as 'queer', 'curious', 'peculiar', and so forth. He also attaches weight to which points of view are currently fashionable or unfashionable. In the closing portions of the book, the author extols the virtues of contemporary Oxford philosophy, particularly as exemplified by Austin. Because of its overall brevity, the book is highly selective and generally sketchy in its treatment of particulars. All things considered, I do not find much merit in it.

B. DUNHAM

The Structure of Chemistry. By E. F. Caldin. Sheed and Ward, Ltd., 1961. Pp. 49. 3s. 6d.

IN this essay Dr Caldin has given a concise account of the Structure of Chemistry as the subject appears to a physical chemist; an account which could be recommended as useful reading for any student of chemistry.

The second stated aim of the work is to suggest points of contact between

chemistry and contemporary accounts of scientific method. In this context Dr Caldin points out that in chemistry theoretical advances depend closely on experiment and that the methodology of this process has been neglected. He also suggests that a consideration of the structure and procedures of chemistry could contribute to discussions of scientific method in several ways; for example in considering whether theories are explanatory as well as instrumental and in considering the status of such theoretical entities as atoms.

It does seem a pity that after showing where the points of contact lay Dr Caldin did not develop his views more fully.

W. F. COULSON

Pain. By F. J. J. Buytendijk.
Hutchinson, London. Pp. 190. 25s.

THIS book was first published in Dutch in 1943 and now appears in an English edition with a supplement aimed at bringing it up to date.

In the first of the book's four chapters the author points to the contemporary lack of interest in what he considers to be the central problem of pain, that of the nature of the experience. Both medicine and psychology must concern themselves with pain and yet the former proceeds by attempting to discover more efficient means of blotting out the experience while the latter neglects the experience in its emphasis on the sensory physiology of the phenomenon. The present book attempts to redress the balance. Professor Buytendijk believes that pain is experienced as a limitation of the sufferer's personal freedom, as a reminder of his helplessness, and as a foreshadow of death itself. In Léon Bloy's words, ' Suffering passes, but the fact of having suffered never passes '.

The next two chapters are devoted to the physiological basis of pain sensation and to the evidence for the view that animals experience pain. These are the best chapters in the book, although it is unfortunate that there is no mention of the important work of Thompson and Melzack on the influence of early environment on the adult's perception of pain. Most of the references are to the older continental literature and the book may prove to be a most convenient source for these.

The final analysis of the experience of pain is much less satisfactory. A deal of space is taken up with the classificatory problem of whether pain is a feeling or a sensation. Perhaps it is the author's impatience with the naïveté of much of the research he has earlier reviewed that leads him to overvalue the rewards of the experience itself and to bolster up this algophilia with religious references to ' blessed suffering '. However, the many

perceptive comments should not be lost in the emotive language which surrounds them and it is for these that the book is likely to be valued rather than for its attempt at a scientific contribution.

D. W. FORREST

Sensation and Perception. By D. W. Hamlyn.
Routledge and Kegan Paul, London 1961. Pp. xii — 210. 25s.

SUBTITLED ' A History of the Philosophy of Perception ', this book outlines what the more prominent philosophers from the Presocratics onwards have said on the subject, considering especially whether they have properly distinguished sensation and perception. Great labour must have gone into writing it, and the treatment is generally clear and reliable, about seven to twelve pages being devoted to the best known thinkers and less to others. Non-philosophers and students will find the survey useful and profit by the handy bibliography, but the author hopes for more—that the survey will give clearer insight into the nature of the problems and the way to their proper solution. Unfortunately if he is right as to the solution and even as to what are the proper questions to ask on perception, one may well wonder whether the traditional philosophers were not so far off course that one should steer clear of them. In practice, however, his own positive opinions are kept mainly to the last chapter and do not unduly colour his historical survey. His own view of the relation of sensation and perception (p. 196) is that the first is a necessary condition of the second, but that ' thi is so only because there is a *conceptual* link between [them] '; in particula the having of a sensation is not a part of perceiving, and perception is no a process but is in a special category as an epistemological concept. I find this last chapter so condensed that it is difficult to grasp, at least it does not convince me of the error of the older approaches to perceiving, nor will it appeal to the modern neurologists who are so curtly dismissed on page 185; but it may well be endorsed by followers of Wittgenstein.

R. J. HIRST

ANNOUNCEMENTS

British Society for the Philosophy of Science

Seventh Annual Conference: 21st-23rd September 1962

A conference will be held at Tetley Hall, Leeds, from Friday evening to Sunday afternoon 21st-23rd September. The conference fee will be £3 15s. 6d. for full residence and gratuities, this includes 10s. registration fee. The programme will be as follows:

Friday evening:	'The Idea of a Physical Quantity'
Chairman	Professor R. B. Braithwaite
Speakers	Dr J. R. Ravetz and Dr H. V. Stopes-Roe
Saturday morning:	'Fundamental Particles and Identity'
Chairman	Dr C. W. Kilmister
Speakers	Dr H. R. Post and Dr P. W. Higgs
Saturday evening:	'Kant's Regulative Principles and their Application to Science'
Chairman	Professor S. Körner
Speakers	Mr J. Bennett and Dr G. Buchdahl
Sunday morning:	'The Nature of Law in Social Science'
Chairman	Mr J. W. N. Watkins
Speakers	Mr P. G. Winch and Mr J. G. H. Newfield.

Those wishing to attend are asked to write to the Conference Secretary, Mr F. T. C. Harris, Department of Biology as Applied to Medicine, Middlesex Hospital Medical School, London W.1. not later than 11th September.

Meetings of the British Society for the Philosophy of Science

1961

9 October:	Dr B. Mayo, 'The Dilemma of Determinism'
6 November:	Dr W. H. Thorpe, 'The Idea of Emergence in Biology'
11 December:	Professor L. S. Penrose, 'A Theory of Self-reproducing Mechanisms'

1962

15 January:	Presidential Address: Professor R. B. Braithwaite, 'Is there an Ordering of Theoretical Concepts?'
12 February:	Professor O. Frisch, 'Observation and the Quantum'
19 March:	Annual General Meeting. Professor C. F. A. Pantin, 'Scientific Method and Intuitive Inference'
7 May:	Mr A. W. Haslett, 'Limits of Explanation'
28 May:	Dr E. F. Caldin, 'The Logic of Discovery in Chemistry'
18 June:	Professor H. Fain, 'Some Problems of Causation'

ABSTRACTS

Dialectica, 1959, 13, Nos. 3/4

J.-Cl. Piguet, ' Note sur la philosophie d'inspiration scientifique '

Starting from the correlativity of the subject and the object in cognition, and from the modal character of all cognition, the author criticises negatively the way in which the ' science-inspired philosophy ' solves the problem of the relations between science and philosophy.

J. K. Feibleman, ' The Logical Structure of the Scientific Method '

If science is still taught by the apprentice method, it is because the scientific method has never been properly abstracted. Full abstraction discloses that there is only one scientific method. It has six well-defined stages: observation, hypothesis, experiment, theory, prediction, and control.

G. E. Müller, ' Dialectic—the Logic of Philosophy '

Dialectic defines itself as a unity of contrary and contradictory opposites; as such it is the logic of systematic philosophy as distinct from formal logic of object sciences.

H. M. Tiebout, Jr., ' Subjectivity in Whitehead '

Whitehead's analysis of human selfhood does not involve a vitalistic reductionism. Whitehead does not derive his doctrine of the human self from a general cosmological analysis, but arrives at his general cosmology by generalising from his description of human selfhood. It is existentialist rather than vitalist.

Dialectica, 1960, 14, No. 1

A. Vandel, ' Un humanisme scientifique '

L'auteur retrace rapidement les différents aspects sous lesquels apparaît la pensée humaine, au cours de son histoire. Il souligne l'importance que prend dans le monde contemporain, le mouvement scientifique. C'est dans un esprit nouveau que se construisent aujourd'hui les grandes synthèses qui essaient de rassembler et d'interpréter les connaissances humaines. La science a toujours tendu vers la recherche de l'unité.

M. Gex, ' Défense de la philosophie d'inspiration scientifique. Réponse a la *Note sur la philosophie d'inspiration scientifique* de M. J.-Claude Piguet '

1. The subject-subject relation proves that it is wrong to assert that it is only the subject-object relation that should be real as constituting the world of things.

2. There is no ' pure philosophy ', as philosophy should take into account all the modes of approach to reality rather than confine itself to proud isolation.

3. The same constituting reason apprehends different modes of being and expresses them in different languages.

4. It is the same reality under two different modalities but closely related to each other, which is apprehended by science and exercises an influence on us.

G. A. Blair, ' Science, Sufficient Ground, and the Possibility of Metaphysics '

The scientist's basic question, ' What is going on? ' shows that he finds an apparent unintelligibility in nature, and that he cannot accept nature as unintelligible. In his theory he may assert the existence of entities, even those unobservable in principle, because he knows there must be in nature something with at least the properties necessary to account for the observed facts. But science is too specific to answer all ' What is going on? ' questions, and therefore there ought to be a more generalised science, metaphysics, which would employ a generalisation of scientific method.

I. Supek, ' Escape of Philosophy into Linguistic Depths '

Like Hume's philosophy, modern positivism explains the world by means of a series of sensations. Russell holds that each proposition contains elementary facts. Empirical verification in physics, which is the paragon of neopositivists, is more complex.

The positivists escape epistemological problems by sacrificing their ego. The contemporary English philosophy has lost its ego in the theory of knowledge and has thus ceased to be a historical subject in the present situation.

Philosophy of Science, 1959, **26**, No. 2

P. Moon and D. Spencer, ' Mach's Principle '

Recession of the galaxies indicates a repulsive force and suggests that Newton's formulation of gravitation is not complete. A possible modification is proposed, and this Neo-Newtonian equation allows a quantitative treatment of Mach's principle. It also limits the velocity of matter to c and gives a correct prediction for the perihelion of Mercury.

Philosophy of Science, 1959, **26**, No. 3

H. S. Leonard, ' Interrogatives Imperatives, Truth, Falsity and Lies '

This paper aims to establish three major theses: (1) Not only declarative sentences, but also interrogatives and imperatives, may be classified as true or false. (2) Declarative, imperative, and interrogative utterances may also be classified as honest or as dishonest. (3) Whether an utterance is honest or dishonest is logically independent of whether it is true or false.

O. Bird, ' Peirce's Theory of Methodology '

Peirce conceived of methodology, or methodeutic, as he preferred to call it, as one of the three major parts of logic taken broadly—the other two being the theory of signs and formal logic. Unlike these two, however, his theory of methodology remained mostly programmatic, and there is little more than fragmentary suggestions

about it scattered through his writings. But by gathering them together and pursuing their insights, it is possible to indicate how he might have divided and developed it: (1) The nature of scientific discourse and how it differs from non-scientific. (2) The logic of enquiry, both heuristic and systematic, according to the modes of argument as deductive, inductive, or abductive (i.e. hypothesis) or a combination of all three. (3) The assurance of science considered in the factors that thwart or promote inquiry.

P. Caws, 'The Functions of Definition in Science'

Definition is viewed in this paper as a cohesive element of theory, providing links between scientific constructs. The problem is approached first in terms of three orders—the historical, the logical, and the heuristic—in which the structure of science may be put together; a study of these is necessary if difficulties about priorities of definition are to be resolved. The main part of the paper is devoted to an exercise in theory-construction which illustrates the five principle functions of definition—the grounding of constructs in observation, their descriptive interrelation, the development of logico-mathematical calculi, the interpretation of these calculi, and the provision of precise, quasi-mathematical relations between the constructs themselves. The theory thus constructed is represented diagrammatically. It is shown that the analysis may be simplified, in general terms, by the use of two new categories, 'internal' and 'external' definition; and that this innovation may prove helpful in clarifying some traditional obscurities, and in preserving a necessary balance between a purely logical and a purely empirical approach to the philosophy of science.

I. F. Laucks, 'Was Newton Right after All?'

Special relativity was based on the theorem that time is affected by motion. Einstein's proof of this was an imaginary experiment with clocks, using light as a synchronising signal. He has said that the kind of signal was immaterial. Subsequent interpreters have stated that sound signals could just as well have been used.

Today any aeroplane passenger's watch denies Einstein's mathematics, *had he used sound*. The Lorentz-Einstein transformation equations in which the speed of sound is substituted for the speed of light are unthinkable. But if they do not hold with sound as the signal, no more do they with light, for the process is identical. The transformation equations for space are based on the variability of time, and are therefore likewise denied.

G. Schlesinger, 'Two Approaches to Mathematical and Physical Systems'

It is commonly the case that a problem concerning a mathematical or physical system can be solved in two quite different ways—by an internal or an external approach. For example, the area of a triangle can be found by integration or by showing it to be half that of a certain rectangle. In general, the first approach is, to analyse the given system into component parts, and the second approach is to deal with the system as a whole.

In cases where solutions to physical problems obtained according to these two approaches are equally valid, and are equally good as explanations, scientists prefer the internal approach. The reasons for this preference are examined.

R. W. Workman, ' Is Indeterminism supported by Quantum Theory? '

Two initially different arguments for indeterminism are often based either upon the Uncertainty Relations or the statistical interpretation of the wave equation of quantum mechanics. Both arguments ultimately involve three factors: (1) the assumption that elementary entities are enough like classical particles for it to make sense to say they are either determined or indetermined, (2) the fact that no exact measurements are possible of quantities supposed to characterise elementary entities, (3) the pragmatic supposition that determinism is false unless exact predictions are theoretically possible.

If it is legitimate to use (3) to prove indeterminism, then an equally legitimate argument can be based upon (2) which denies (1). Thus, it is doubtful that quantum mechanics supports indeterminism, though it may show that the concepts of ' determined ' and ' indetermined ' are inapplicable to the world.

G. Matheson, ' The Antinomy of Designation '

A new semantical antinomy, *the antinomy of designation*, is introduced into a metalanguage M with respect to a modal object language L. The consequences of this are discussed.

Philosophy of Science, 1959, **26**, No. 4

H. S. Leonard, ' Authorship and Purpose '

This paper approaches a theory relating authorship, meaning, and purpose by semi-formalised developments of two ' presupposed theories ': of purposeful behaviour and of sign-reading.

R. V. L. Hartley, ' A Mechanistic Theory of Extra-atomic Physics '

A theory analogous with the kinetic theory of heat is described, in which the role of heat is shared by all the phenomena of extra-atomic physics, including quantum electrodynamics, gravitation, and relativistic mass. The role of the randomly moving molecules, as a mechanical model, is taken for all of these by a single model, consisting of a turbulent, dissipationless liquid, the motion of which conforms to Newtonian mechanics. This model is capable of supporting spherical standing waves which are taken to represent elementary particles. The mechanical counterparts of the physical equations are close approximations to those of the model.

V. Hinshaw, Jr., ' Determinism versus Continuity '

Prompted by Alfred Landé's appraisal of individual indeterminacy in both ordinary and quantum games of chance (*Mind*, n.s. 67: 174–81, April, 1958), this paper suggests an alternative assessment in terms of the model-structure of physical theory. The author indicates how the *partial* interpretation of the kinetic and quantum models limits us to statistical generalities—to limited ' areas of relative chance '. The alleged indeterminism of physics thus resides in the model and its partial interpretation in the sense that the statistical nature of statements about ' the next throw ' or about individual particles is built into the model that enables the scientist to talk at all about such outcomes or entities.

ABSTRACTS

N. R. Hanson, ' Five Cautions for the Copenhagen Interpretation's Critics '

Within the past decade there has grown an acute and highly articulate group of critics of the orthodox interpretation of quantum theory—the so-called 'Copenhagen Interpretation'. The writings of people like Bopp, Janossy, and particularly Bohm and Feyerabend, must be taken very seriously indeed. The future of some important discussions in the philosophy and the logic of science rests with these individuals. But they have, in their own writings, occasionally matched the inelegancies of Bohr and Heisenberg with as many inelegancies of their own. The present paper is meant to present a quintet of considerations which may possibly lead to a reassessment of the issues between Bohr, Heisenberg, and their critics, especially Bohm and Feyerabend.

J. R. Gregg, ' On Deciding whether Protistans are Cells '

There is a biological controversy of long standing between proponents of the view that all organisms of a certain class have at least one part that is a cell and proponents of the contradictory view that some organisms in the same class have no parts that are cells. The controversy is considered from the standpoint of the methodology of explication. It is concluded that on the grounds of prevalent biological usage, precision, utility, and generality the first view may be defended successfully against attacks that have been made upon it.

R. W. Dettering, ' Linguistic Superfluity in Science '

The lessons of ' logical empiricism ' still need learning by scientific philosophers who continue to give ultimate descriptions of ultimate things. The effort to force scientific philosophy into an ontology, as shown by a recent example, turns out to be only a conventional way of talking, rather than a legitimate appeal to something in the universe. Recent examples of scientific metaphysical descriptions of ' reality ' reveal a deceptive linguistic superfluity in no way necessary for science or scientific philosophy.



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BRITISH SOCIETY FOR THE PHILOSOPHY OF SCIENCE

THE British Society for the Philosophy of Science is a body of which the purpose is described by the following statement from its Constitution and Rules:

> 'The purpose of the Society is to study the logic, the method and the philosophy of science as well as those of the various special sciences, including the social sciences.'

The Society holds meetings, at which papers are read and discussed, once a month during the academic year. These are held, by kind permission, in University College, London.

Ordinary membership is open to all persons who have been proposed by two members, approved by the Committee, and elected at an ordinary meeting. The annual subscription is 10s. 6d. Those who wish to join should write to the Hon. Secretary, Dr H. R. Post, 18a Christchurch Hill, London, N.W.3.

The Northern Group of the British Society for the Philosophy of Science meets regularly at various centres in the north of England. Those who wish to join it should write to Dr W. Mays, Hon. Secretary of the Northern Group of the British Society for the Philosophy of Science, Dept. of Philosophy, The University, Manchester, 13.

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THE BRITISH JOURNAL FOR THE PHILOSOPHY OF SCIENCE

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DETERMINISM OR INDETERMINISM IN MICROPHYSICS *

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1 *Philosophical Presuppositions*

IN this paper I am going to assume that the Causal Principle, the sentence 'Every event has a cause', may be used to assert something which is *either true or false*, even although we can never have conclusive empirical evidence for saying which.

In doing so, I shall be dissenting from present day philosophical fashion which has it that this sentence ought rather to be construed as a heuristic maxim or methodological principle only, i.e. as a kind of prescriptive utterance.[1] On either interpretation, it should be noted, it would be held that the sentence 'Every event has a cause' is, for purely logical reasons, both unverifiable and unfalsifiable. Where they differ is over the question whether its empirical undecidability—its unverifiability-cum-unfalsifiability—counts as a sufficient condition of its being neither true nor false. Thus, according to the prescriptive interpretation, any sentence—such as the Causal Principle—which is neither verifiable nor falsifiable is either wholly meaningless or else has that non-cognitive kind of meaning only which is possessed by prescriptive utterances such as 'Let us look for causes': at all events, such a sentence, it would be said, cannot 'express a proposition' or 'make a statement'. This interpretation, I believe, derives from an implicit adherence to an Empirical Decidability or broadly operationalist theory of cognitive meaning. On my view, however, there is no contradiction involved in saying that it is either true or false that

* Received 13.vii.61

[1] See, for example, A. J. Ayer, *The Foundations of Empirical Knowledge*, London, 1940, p. 220; J. Hospers, *Introduction to Philosophical Analysis*, New York, 1953, p. 261; A. Pap, *Elements of Analytic Philosophy*, New York, 1949, p. 226

every event is causally determined even although we can never know which. I do not deny that the Causal Principle may function within the field of scientific enquiry more as a regulative principle than as a theory whose truth or falsity is at stake, nor do I wish to under-rate its importance in this role. What I deny is simply that its regulative role is its only possible one and that it therefore cannot sensibly be asked whether the thesis of Causal Determinism[1] is true or false. Clearly, unless this at least is conceded to me, the question which is at issue in this paper—whether microphysics establishes the falsity of Causal Determinism—cannot even arise.

As I have said, I am going to assume that this question can arise. My aim is to answer the question, and to answer it in the negative by arguing that since the empirical evidence does not *entail* Causal Indeterminism it is logically consistent with Causal Determinism. (I shall not argue, what seems to me mistaken, that this evidence entails the truth of Causal Determinism.)

2 *Rival Interpretations: Epistemic and Ontological*

The immense success of classical, Newtonian, mechanics in explaining the behaviour of macrophysical systems and in predicting their future states has long seemed to the Determinist sufficient con-

[1] Two points need to be made here. (1) I think it essential to distinguish between Causal Determinism (an ontological thesis about the *existence* of causes for all events) and Predictive Determinism (an epistemic thesis about the *knowability* of such causes and the consequent *predictability* of future events). Clearly, the truth of Causal Determinism is a necessary condition of the truth of Predictive Determinism. But since it is not also a sufficient condition, it is a fallacy to argue—as many have done—from the denial of strict predictability to the denial of Causal Determinism.

(2) The view that the sentence 'Every event has a cause' has the status of a factual proposition could be argued at length. But not here. One brief argument must suffice: Although there is no verifiable proposition with which the Causal Principle is incompatible (see S. Hampshire: 'Multiply General Sentences', *Analysis*, 10.4), it does not follow from this that there is no factual proposition with which it is incompatible, that it is compatible with any and every state of affairs (claimed by G. J. Warnock: 'Every Event has a Cause', *Logic and Language*, (ii). On the contrary, it is incompatible with the state of affairs asserted by its syntactical contradictory 'Some events are uncaused', which, though unverifiable, does at least seem to be a genuine factual proposition. (At least, it is no more disqualified than any strictly universal, and unverifiable, 'law of nature'.) And whatever is inconsistent with a factual proposition must itself be a factual proposition. Its unverifiability-cum-unfalsifiability makes the Causal Principle an unusual statement of fact: but it is a statement of fact for all that.

firmation of his belief that every event has a cause. Certainly, our everyday observations of changes or events in the macrocosmic physical world seldom, if ever, lead us seriously to doubt that for every event there is, somewhere or other, another event or set of events which causes it to occur. Something occurs which demands a causal explanation; we look for causes of the change and, for the most part, we find them. If we do not find them we can always save the Causal Principle by supposing that more accurate observations, more detailed knowledge of the field of change, would reveal the set of conditions which is both necessary and sufficient to produce it. Nor is this just an idle supposition. For in more cases than not it is subsequently confirmed by experience when, after further investigation, we do in fact discover the invariant relation which counts as evidence of the operation of a causal law. It seems, indeed, that our observations of the behaviour of large-scale physical objects—including human beings as well as animals, billiard-balls, and planets—constantly confirm, but can never conclusively disconfirm the contention that every change or event has a cause.

But a very different story is nowadays told us by those physicists who claim to have experimental acquaintance with the microcosmic world with which quantum mechanics deals. Here, we are told, experiments reveal that the domain of small things is ' full of the most capricious irregularities, unlike the regularities in the domain of ordinary experience '.[1] It is said that here the rigid laws of classical causality no longer hold sway but are replaced by what Lenzen describes as the ' irreducibly statistical ' laws of quantum theory. Strict predictions give way to statements of probabilities, and the Causal Principle of Determinism gives way to the Heisenberg Principle of Indeterminacy. The latter is supposed to point to an ontological, not merely epistemic indeterminism in the behaviour of the elementary particles of which all matter is ultimately composed. That is to say, the behaviour of these particles, as described by Heisenberg's formulae, is taken to be such that not only have we not, as yet, discovered any causal laws to which it is subject, but we never will, for no such laws exist. It is contended, also, that since all physical objects are made up of these randomly behaving particles, even the motions of large-scale objects must in principle be undetermined, and accordingly the apparent continued success of classical mechanics in predicting these motions is

[1] P.W. Bridgman, *Reflections of a Physicist*, New York, 1950, p. 129

explained in terms of statistical probabilities. To sum up, the claim made by the orthodox exponents of quantum physics is that ' the determinism of classical physics has been abandoned in modern quantum mechanics.'[1]

But is it the case, as claimed, that the discoveries of contemporary physics have shown that Causal Determinism is false, i.e. that there are at least some events which are uncaused?

Even physicists themselves are divided on the issue. For although the orthodox interpretation of the experimental evidence given by the majority of quantum theory's leading exponents, e.g. Heisenberg, Bohr, and Born, is uncompromisingly indeterministic, there is a group of theoretical physicists of no less standing, e.g. Einstein, Schrödinger, and de Broglie, who claim that the experimental evidence warrants only the epistemic conclusion that there are at present limitations on our ability to detect causal laws at the quantum-mechanical level, not the ontological conclusion (of the orthodox theory) that such laws do not exist. These rebel physicists confidently expect that the time will come when, in de Broglie's words: ' we will be able to interpret the laws of probability and quantum physics as being the statistical results of the development of completely determined values of variables which are at present hidden from us.' ' The idea of chance . . . ', he claims, ' comes in at each stage in the progress of our knowledge, when we are not aware that we are on the brink of a deeper level of reality which still eludes us.'[2] Similarly, Einstein remarks: ' I am . . firmly convinced that the essentially statistical character of contemporary theory is solely to be ascribed to the fact that this theory operates with an incomplete description of physical systems.'[3] Their dissatisfaction with the orthodox interpretation stems both from a critique of its epistemological presuppositions (inherited from the positivism of Ernst Mach) and from their recognition that ' quantum physics has found itself for several years tackling problems which it has not been able to solve and seems to have arrived at a dead end '. But this is not all. For some years, it appears, younger physicists like Bohm and Vigier, have been trying to formulate a theory which will in fact account for the statistical descriptions of the behaviour of particles

[1] H. Reichenbach, *The Direction of Time*, Berkeley, 1956, p. 17

[2] L. de Broglie, Foreword to D. Bohm's *Causality and Chance in Modern Physics*, London, 1957, x, xi, pp. x, xi

[3] A. Einstein, ' A Reply to Criticisms ', *Albert Einstein: Philosopher-Scientist*, (ed. P. A. Schilpp), New York, 1951, p. 666

at the quantum scale of 10^{-8} to 10^{-10} cm. in terms of a deterministic substratum of hidden parameters in the range below 10^{-13} cm. Now, it may be that no such theory will ever successfully be constructed, or, even if constructed, experimentally confirmed. But the epistemological critique on which it is at least partly based has already done much to break down the quite arbitrary *a priori* assumption—supposedly proved by a theorem of von Neumann—that the current theory is absolute and final. Indeed, recent reports suggest that Bohm's revolutionary proposals are at last receiving the serious consideration which they deserve.

Now it is on epistemological, and, more broadly, logical grounds that I too, wish to dispute the orthodox ontological interpretation of quantum theory. I have already indicated that there are reasons for holding the logical properties of the statement 'Every event has a cause' to be such that no conceivable evidence, even if it carries with it the supposedly unimpeachable authority of contemporary physics, can possibly falsify it. The non-discoverability of the cause for a particular event or class of events does not entail the non-existence of such a cause. Again, in order to falsify 'Every event has a cause' we should have to verify its contradictory, a negative existential statement of the form 'There does not exist any cause for this or that event'. But negative existential statements are completely general and for that very reason are universally allowed to be *not* empirically verifiable. It follows that in whatever sense it can properly be said that a completely general statement is unverifiable, it can also properly be said that 'Every event has a cause' is unfalsifiable. It is therefore not open to anyone to suggest that more careful consideration of all the experimental evidence which microphysicists have in mind might legitimise their inference from non-discoverability to non-existence. For if the above argument is sound, then there is, as I have just said, no conceivable evidence whatever which could warrant this indeterministic conclusion; and *a fortiori* no considerations about the continuity or discontinuity of quantum processes could ever establish it. This argument, I believe, is conclusive. But in order to escape the charge of mere *a priorism*, I now want to show, in more detail, that anyone, whether he be a philosopher or a physicist, who supposes that the experiments of quantum mechanics establish the falsity of the Causal Principle, is guilty of serious logical confusions in his interpretations of whatever empirical data these experiments supply. And if called upon to justify my presuming, as a mere philosopher, to criticise a

physical theory, I should answer, first, that *I do not dispute any of the experimental findings of quantum physics—how could I?—but only the inference by which these are supposed to lead to the ontological indeterminism of the orthodox interpretation*; and secondly, that the question whether these inferences—like any others—are valid or not is a matter for logic alone, not physics, to decide. Indeed, I should go even further, and claim more generally that a philosopher is not only entitled but is obliged to point out logical confusions and faulty reasonings in any field of enquiry whatever—physics included. My position may be stated in the words of Sidney Hook who declares: ' Normally I should be reluctant to speak of the principle of indeterminacy in the presence of physicists, but having recently read what eminent physicists have written about philosophy, I feel absolutely shameless.'[1]

It is my intention, then, to examine, on the one hand, the experimental evidence which forms the basis of the new mechanics and, on the other hand, the ontological interpretation which this evidence is thought to warrant, and then to enquire into the validity of the principle of inference which is employed to get from the first to the second. It will be shown that the argument involved is identical with that which I call an ' epistemic-ontological confusion ' and that it derives from an explicit adherence to an Empirical Decidability theory of meaning.

The new physics takes its name from Max Planck's discovery of the quantum law, viz. a law of nature, operating in the microphysical domain, which asserts that energy varies only discontinuously, i.e. always by multiples of a quantity known as the ' quantum of energy ', and never by fractions of this quantity. Now this law, it has been found, imposes limits on the precision with which we can measure the parameters—initial conditions—of systems at the quantum-mechanical level. Suppose, for instance, that we want to determine experimentally the precise state of a simple microphysical system, e.g. the position and momentum of a single particle at a given instant of time. Then the only way in which we can make the necessary measurement is by using some form of energy sufficient to produce a detectable reading in our measuring apparatus. And this is where our trouble begins. For we are at once faced with a dilemma. We must select, as our medium, either relatively high-frequency forms of energy or relatively low-frequency forms of energy. If we choose to use, for example,

[1] S. Hook, 'Necessity, Indeterminism and Sentimentalism', *Determinism and Freedom in the Age of Modern Science: A Philosophical Symposium* (ed. S. Hook), New York, 1957, p. 170

electro-magnetic waves of high-frequency, then because their wave-length is small relative to the dimensions of the particle we secure fairly good definition of its image in the recording instrument and so are able to determine the particle's position with corresponding accuracy; but at the same time, a high-frequency beam deflects a relatively high amount of energy from the particle thus disturbing or deflecting its momentum by a corresponding amount of energy, i.e. the so-called 'quantum-effect' is pronounced. Yet if, on the other hand, we use a low-energy beam, we can minimise this quantum-effect, but only at expense of the accuracy of our measurements of the particle's position, for since the wave-length will be large relative to the dimensions of the particle, the diffraction-effects will be pronounced. In effect, we are faced with a choice between quantum-effect and diffraction-effect. And this means that our ability to determine accurately the particle's momentum is reciprocally related to our ability to determine accurately its position. Or, more generally, it means that the more accurately we measure one parameter the less accurately we can measure the other parameter which is complementary to it. (This is deducible from Bohr's 'Principle of Complementarity'.)

Now this result admits of precise mathematical statement. It is given in Heisenberg's uncertainty relations:

$$\Delta q \,.\, \Delta p \geq h;\ \Delta E \,.\, \Delta t \geq h$$

These formulae are to be interpreted as equivalent descriptions of the same state of affairs. Each asserts that the numerical product of the uncertainty of one parameter and the uncertainty of the other—whether we are considering the conjugate parameters of position (q) and momentum (p) appropriate to the particle-picture, or those of energy (E) and time (t) appropriate to the wave-picture—is always greater than, or equal to, Planck's quantum of action (the constant h). In other words, these formulae assert that Planck's constant represents the theoretical upper limit to the accuracy with which the state of a microphysical system can be determined.

Summarising the position, we may say that the experimental situation described establishes the following facts: (1) that the initial state of a microphysical system cannot be fully determined; (2) that it is impossible to formulate strictly causal laws of the form 'If p_1 and q_1 at t_1, then p_2 and q_2 at t_2', since determinate values cannot simultaneously be given for the variables 'p' and 'q' (this is deducible from (1)); and (3) that it is impossible to make strict predictions of

the future state of such a system (this is deducible from (2) and hence, also, from (1)). These three claims comprise the *epistemic* ' interpretation ' or description of the experimental data.

So far nothing has been said which is in any way philosophically objectionable. Indeed, in all this, the philosopher has been forced to stand on the side-lines. For all that we have been given is a straightforward report of what the quantum physicist finds when, in the laboratory, he attempts to determine the parameters of physical systems, and what he is able to do—or, more significantly, not to do—with the information he gets; and it is clearly outside the province of philosophy, and the competence of most philosophers, to dispute anything of this kind. But it is when the orthodox quantum physicist starts to give his *interpretation* of these empirical findings, when he attempts to explain their significance or say *why* it is that he cannot determine initial conditions, formulate causal laws, and issue predictions, in the microphysical domain, that the trouble arises.

For the orthodox theorist is not content simply to describe the experimental facts in terms of what we know and do not know about the states of affairs in microphysics: he attempts to go further and *explain* this purely epistemic indeterminacy in terms of a real or ontological indeterminacy in the states of affairs themselves. As de Broglie says, he asserts that our uncertain and incomplete knowledge ' is the result of a real indeterminacy of the physical states and of their evolution '.

The claims of the *ontological* interpretation are as follows: (1) It is claimed, first, that it is not merely a case of the initial states of microphysical systems not being determinable by us but rather that microphysical systems just do not *have* determinate initial states, e.g.—in the case of a particle—that it does not have a determinate position or a determinate momentum. Thus Bridgman[1] reports Bohr as arguing that if a particle's parameters cannot be known simultaneously then the particle does not have them simultaneously. And Hutten writes: 'We must say with Heisenberg that " this indeterminateness is to be considered as an essential characteristic of the electron, and not as evidence of the inapplicability of the wave picture ".'[2] This claim that the initial state of a microphysical system is logically indeterminate is implied by Born's statistical interpretation of Schrödinger's psi-

[1] P.W. Bridgman, 'Determinism in Modern Science', *Determinism and Freedom in the Age of Modern Science*, etc. (ed. S. Hook), op. cit., p. 48

[2] E. H. Hutten, *The Language of Modern Physics*, London, 1956, p. 180

function. For if the latter is regarded as a complete description of, say, an individual atom, then it cannot be said that the atom has a definite, i.e. determinate, disintegration-time, but only that its disintegration-time is vague or indeterminate; from which it follows that it can be predicated of the atom only within a certain range of statistical probabilities. This is why Einstein reports that he is 'firmly convinced that the essentially statistical character of contemporary quantum theory is solely to be ascribed to the fact that this theory operates with an incomplete description of physical systems'. He rejects the view that an indeterminate (statistical) description can be a complete one, and concludes that Schrödinger's psi-function 'is to be taken as the description, not of a singular system, but of an ideal ensemble of systems'.[1]

(2) The second claim is that the reason why we cannot determine—in the sense of 'ascertain'—what causal laws correlate one state of a microphysical system with another, later, state, is simply that no such laws exist. Bohm[2] describes this claim thus:

> In the usual interpretation of the quantum theory, the precise magnitudes of the irregular fluctuations in the result of individual measurements at the atomic level are not supposed to be determined by any kinds of causes at all, either known or unknown. Instead, it is assumed that in any particular experiment, the *precise* result that will be obtained is *completely arbitrary* in the sense that it has no relationship whatever to anything else that exists in the world or that ever has existed. [2]

And Hutten makes it quite clear when he writes: 'Although the equations of quantum mechanics have the same mathematical form as the classical equations, the deterministic conception of causality has to be abandoned.'[3] The claim is that quantum mechanics has shown the Causal Principle to be false, since it has established the truth of its contradictory 'Some events are uncaused', if not its contrary 'No events are caused'. For most quantum theorists the Causal Indeterminism here asserted follows simply from the Logical Indeterminism of (1). Thus, if determinate states of microphysical systems do not exist, there can be no causal laws relating such states. However, Causal Indeterminism need not be asserted on these grounds. There would be no inconsistency involved in conceding that, say, a particle has a determinate position and momentum both at time t_1 and at time

[1] op. cit., pp. 666, 668 [2] op. cit., p. 87 [3] op. cit., p. 181

t_2 while denying that there is any law of invariable succession whereby these determinate states are correlated with each other.

(3) Finally, it is claimed that Heisenberg's ' Principle of Uncertainty ' (or ' Indeterminacy ', as the orthodox theorists prefer to call it) reflects, not merely an ignorance on our part of causal laws and initial conditions, but a *real unpredictability* of events at the quantum mechanical level. This is equivalent to the thesis of Predictive Indeterminism but, so far as the unpredictability of micro-events is said to be 'real' and not merely an epistemic matter of our lack of knowledge, it reduces to the thesis of Causal Indeterminism, viz. that there are in fact no causal laws the knowledge of which would permit strict predictions of the future states of microphysical systems.

It seems to me that there is much in all this that is philosophically and logically objectionable—and, I should have thought, scientifically objectionable too. For in what way, we want to know, is this ontological interpretation warranted by the experimental facts? The answer, as I see it, is that it is not warranted in any way at all. Thus, I hope to show that as an explanation of these facts it is unnecessary and that as an inference from them it is illegitimate. Let us now examine this suggestion for each of the two ontological indeterministic claims of the orthodox theory. (The thesis of *real* predictive indeterminacy need not be considered separately since it reduces to one of these.)

3 *Critique of the Ontological Interpretation*

(1) The fundamental claim, from which the others are deducible, is that events and states of affairs in the microphysical world are in themselves logically indeterminate. Thus Hutten[1] writes:

> . . . the deterministic conception of causality has to be abandoned. We say this for the reason that the microphysical events are essentially indeterminate.

And Bohm[2] describes it in this way:

> . . . as far as any physically observable variables are concerned, it is not to be supposed that these are just ' uncertain ' to us, because we are not able to measure them with complete precision. Rather, one assumes that their very mode of being requires them to be indeterminate.

But, as we shall now see, there are at least four distinct versions of this claim—often confusedly regarded as equivalent.

[1] op. cit., p. 181 [2] op. cit., footnote, p. 85

(*a*) One view is that although microphysical *events*, e.g. the disintegration of an atom, occur at some time or other, they do not occur at any determinate time. This is implied by the assertion that Schrödinger's psi-function gives a complete description of the single system concerned. Putting it in terms of microphysical *states of affairs*, this version asserts that although a particle has some position and some momentum or other, no *determinate* position or momentum can be predicated of it, i.e. the values of these parameters are 'blurred', 'vague' or 'imprecise'. It is neither true nor false but merely indeterminate whether an atom will disintegrate at a specific time or whether a particle has a given position and momentum.

But at least three fatal objections can be brought against this interpretation. In the first place, there can be no conceivable warrant in experience for a statement of this kind. It is granted, by all concerned, that we do not know—cannot determine—with precision the simultaneous values of the conjugate parameters of a microphysical system, but it does not follow from this—as the official theory would have us believe—that these parameters do not *have* simultaneously precise values. The mistake here made is to predicate 'vagueness', 'uncertainty', etc., which are purely epistemic concepts of events and states of affairs.

This brings us to the second objection. It is logical nonsense to allow that such concepts as those of 'position', 'momentum', 'path', 'initial state' and the like, can properly be predicated of something while denying that they can be predicated determinately of that thing. For if, say, 'position' and 'momentum' can significantly be predicated of a particle—if, that is, a particle is the kind of thing that has such properties—then it is not just true but logically true (i.e. logically necessary) that the particle either has or has not this or that determinate position or momentum. Similarly, if atoms are the kind of thing which are capable of decaying at some time or other, then it is logically necessary that any statement asserting this or that specific disintegration-time for a particular atom should be either true or false—and determinately so. That is, it is logically necessary that if an atom decays at some time or other it decays at a definite time. For Einstein, though not for his opponents, this is an unquestionable assumption. He points out, however, that 'such a definite time-value is not implied in the description by the psi-function'. And from this he correctly concludes: 'If, therefore, the individual atom has a definite disintegration-time, then as regards the individual atom, its

description by means of the psi-function must be interpreted as an incomplete description.'[1]

The final objection is that it is just not true, as a matter of fact, that atoms have no definite disintegration-time, or that a particle has no determinate position or momentum. For it is conceded by the theory itself that we can determine, with whatever precision we choose, *either* the position *or* the momentum of a particle, though not, of course, both together. Indeed, this can be deduced from Heisenberg's uncertainty relations which permit the determination of the value of one parameter with complete precision—at the expense, however, of the complete imprecision of the determination of the value of its conjugate parameter.

(*b*) It is sometimes said, more radically, that the whole concept of the 'initial state' of a system is inapplicable to the microphysical systems which are the concern of quantum mechanics, i.e. that such concepts as those of position and momentum are not properly to be predicated of sub-atomic particles at all—whether determinately or indeterminately. This is what is meant when physicists tell us that in the new mechanics, as distinct from the old, the concept of the path of a particle 'has no meaning', or again, that the conceptual scheme of classical deterministic physics 'breaks down' in the new physics. It amounts to saying that it is a category-mistake to speak of electrons and the like as if they were the kinds of things of which position and momentum etc. can be predicated.

But there are a number of insurmountable problems for this view. In the first place, only if by 'particle' the quantum physicist does not mean anything like what we ordinarily mean by that word can he avoid saying that microphysical particles, like macrophysical ones, have at least some position or other and some momentum or other. And what reason could an experiment of any kind afford for saying that microphysical particles are logically or categorically different from macrophysical ones, except that in the case of the former, but not of the latter, the experimental situation precludes exact values of these conjugate parameters being determined simultaneously for them? But from this there follows only the operational impossibility of such precise predication, not the logical impossibility of these particles having such parameters. Again, this particular view is clearly refuted by the fact, already noted, that the theory does in fact permit at least the

[1] op. cit., p. 668

disjunctive predication of 'position' and 'momentum' of microphysical systems. Indeed Heisenberg, it should be noted, even acknowledges the possibility of performing experiments which permit the *precise* determination of the values of these parameters conjunctively, and attempts to reconcile this admission with his uncertainty relations by saying that such precise simultaneous measurements, even if operationally compatible, are theoretically sterile since they do not enable us to predict the particle's future state (as if a statement of initial conditions alone *could* suffice for the purpose of predicting future states!). Popper[1] and Margenau[2] also describe similar experiments which allow the precise calculation of the state of an electron at a given instant or its path between two instants to be made. I am not at all sure that I understand how these experiments can be reconciled with the claims of Heisenberg's Uncertainty Principle; but even supposing that they can, i.e. that they do not refute this Principle, the fact that they are possible at least shows the falsity of the claim that, in the new mechanics, the concepts of the 'initial state' and the 'path' of a microphysical entity have no meaning. Bohr's argument that if the parameters of a particle cannot be known simultaneously then the particle does not have such parameters simultaneously, is not only invalid but its conclusion is false.

(*c*) A third version of the claim that microphysical objects and events are themselves essentially indeterminate is stated by Bridgman[3] as follows:

> Since an object never occurs naked but always in conjunction with an instrument of measurement or the means by which we acquire knowledge of it, the concept of 'object', as something in and of itself, is an illegitimate one.

Thus, he explains[4]:

> . . . if one no longer thinks of an electron as an 'object', but as an aspect of what happens in particular kinds of physical situation . . . the paradox disappears even from such statements as that we can 'choose' whether the electron is to have position or velocity. The electron in and by itself does not 'have' either position or velocity; these pertain to what happens in the whole situation, including the instrumentation.

[1] K. R. Popper, *The Logic of Scientific Discovery*, London, 1959, p. 219
[2] H. Margenau, *The Nature of Physical Reality*, New York, 1950, p. 376
[3] P.W. Bridgman, 'Determinism in Modern Science', op. cit., p. 48
[4] ibid., p. 60

Similarly, Grünbaum[1] writes:

> . . . it is because these classically conceived attributes do not jointly exist to begin with that they cannot be obtained by simultaneous measurement . . . Instead of being simultaneous 'autonomous', attributes of the microphysical object, belonging to it independently of the *particular experimental arrangement* into which it enters, exact theoretical values of conjugate parameters are each only *interactional* properties of an atomic object that is coupled *indivisibly* to a particular kind of observational macro-set-up.

This interactionist thesis of Bridgman and Grünbaum, we are told, ought not to be confused with what Grünbaum describes as the thesis of 'macrophysicalism', viz. the thesis that atomic objects themselves do not have an autonomous existence. What is claimed here is only that certain of the classical attributes of such objects are non-autonomous. The difference, according to Grünbaum,[2] is shown by the fact that

> To assert that the particular pairs of attributes which furnished the mechanical state descriptions in classical physics are neither autonomous nor theoretically *con*junctive but rather interactional and theoretically *dis*junctive does not itself entail that there are no *other* attributes of, say, an electron whose existence is *independent* of whether the electron is observed.

But here our first difficulty arises. Although, as Grünbaum claims, interactionism does not by itself entail macrophysicalism (which he rejects), it is not at all clear that there is any pragmatic or operational difference between them. For Grünbaum does not tell us what 'other' attributes of an electron there might be which are not purely interactional and theoretically disjunctive, i.e. which are autonomous in the required sense: nor are any such attributes ever involved in the microphysical descriptions of quantum theory. But, further, even if interactionism does not *itself* entail macrophysicalism, the latter is deducible from it in conjunction with Bohr's Principle of Complementarity which Grünbaum and Bridgman both accept. For it is asserted by the Principle of Complementarity that there are *no* parameters of microphysical systems which are not reciprocally related in complementary pairs, i.e. which are not 'interactional and theoretically disjunctive'. In other words, an interactionist can avoid the implausible

[1] A. Grünbaum, 'Complementarity in Quantum Physics and Its Philosophical Generalisation', *J. of Phil.*, 1957, 54, 717

[2] ibid., p. 177

ontology of macrophysicalism only if he renounces his belief in the Principle of Complementarity which is usually regarded as an essential part of the official interpretation.

A second defect of interactionism is that the kind of argument on which it rests either begs the question or is fallacious. Consider, for instance, the form in which Bridgman puts it: ' Since an object never occurs naked but always in conjunction with an instrument or the means by which we acquire knowledge of it, the concept of " object ", as something in and of itself, is an illegitimate one.' Bridgman here appears to come dangerously close to macrophysicalism. But this aside, it is plain that, as he puts it, the argument begs the question. For what he takes as his premise—' an object never occurs naked '—is equivalent to his conclusion—that an object does not exist ' as something in and of itself'. And if the argument is altered so as to avoid this circularity it becomes fallacious. For what experimental—or more broadly, experiential—evidence could there ever be for an ontological conclusion such as this? Certainly, experience can tell us that an object never is *observed* ' to occur naked but always in conjunction with . . . the means by which we acquire knowledge ': indeed, this is a truism. But it does not follow from this that an object ' never occurs naked ', i.e. never exists ' as something in and of itself '. Such a conclusion—far from being a truism—is certainly false. Besides, it could never derive any support from experience.

Further, even if we were to suppose the thesis of interactionism to be true, it would not in the least count against the truth of the Causal Principle. That it would upset the simple ' Spectator-theory ' of knowledge is undoubted. That it would necessitate the revision of our classical conception of such parameters as position and momentum as autonomous attributes of microphysical objects is probable. But it would not have the slightest tendency to falsify the claim that there are precise conditions for the occurrence of all individual events and precise parameters determining the state of every object—whatever the ontological status of events and objects may be—such that if these were known together with the relevant causal laws of their development, it would be possible to predict future events and future states of affairs.

(*d*) Finally, I want to give brief attention to the macrophysicalist thesis that there are no determinate states of microphysical systems since the latter do not exist independently of being observed or even, perhaps, do not exist at all. The importance of this thesis, which is asserted by both leaders of the orthodox ' Copenhagen ' interpretation,

Bohr and Heisenberg, follows from the fact that, according to Bohr himself ' the renunciation of the ideal of causality in atomic physics which has been forced upon us is founded logically only on our not being any longer in a position to speak of the autonomous behaviour of a physical object '.[1]

What is the genesis and status of this thesis? As I see it, it is a purely philosophical thesis deriving principally from the adoption of a philosophical theory of meaning, viz. an Empirical Decidability, or more precisely, a Verifiability, theory of meaning. Following the lead of Mach and other scientific positivists like Clifford and Pearson, Bohr and Heisenberg set out to rid physics of all its ' metaphysical 'elements —to dispense, in accordance with Occam's Razor, with unnecessary assumptions and unobservable entities. And their instrument in all this was a verifiability or operationalist theory of meaning. Thus Hutten remarks that Heisenberg ' emphasised the need for the empirical verifiability of our sentences; and he demanded that in the description of physical processes only such symbols should be employed as refer, in the most direct manner possible, to observable properties '.[2] It is not surprising, then, that the path taken by so many modern physicists should lead to Berkeleian conclusions. The maxim of macrophysicalism, it appears, is equivalent to Berkeley's *esse percipi*. David Bohm[3] describes the position thus:

> In the usual interpretation of the quantum theory, *an atom has no properties at all when it is not observed.* Indeed, one may say that its only mode of being is to be observed; for the notion of an atom existing with uniquely definable properties of its own even when it is not interacting with a piece of observing apparatus, is meaningless within the framework of this point of view.

Now I should have thought that a thesis of this kind was more befitting for metaphysics than for science—that it was bad philosophy and even worse physics. For the macrophysicalism of the official theory is not a valid inference from or interpretation of, the experimental facts of quantum physics: there is no conceivable observation which could establish either the truth or the falsity of the proposition ' an atom has no properties at all when it is not observed '. Indeed, it is—as I see it—the supreme example of a purely metaphysical thesis masquerading as a physical theory. Paradoxically, it appears that the

[1] N. Bohr, ' Causality and Complementarity ', *Phil. Sci.*, 1937, 4, 290
[2] op. cit., p. 160
[3] op. cit., p. 92

demand for verifiability has led to a theory which is itself unverifiable. But this is just because the demand for the empirical verifiability of our sentences is methodologically sound only so long as it is not interpreted as a criterion of meaning, i.e. so long as it is not taken to involve the meaninglessness of the *un*verifiable. One may quite legitimately decide to assert only what is given by experience, but it is wrong to suppose that this entails the denial of what is not given by experience; certainly '*X* is observed to exist' entails '*X* exists', but '*X* is not observed to exist' does not entail '*X* does not exist'. It is this sort of confusion which I take to be exemplified both in Berkeley's doctrine of *esse percipi* and in all those arguments from experience which purport to establish a real or ontological indeterminateness or indeterminism.

(2) The thesis of Causal Indeterminism, as it is asserted by the official interpretation of quantum mechanics, can now be dealt with more easily. For the principal reason given for holding that there are no causal connections between determinate states of microphysical systems—indeed the *only* reason, according to Bohr—is that no such determinate states exist. And I have already dealt with that contention. If, then, Bohr's claim were correct, there would be no more to be said, for we would successfully have deprived the 'renunciation of the ideal of causality' of what Bohr holds to be its *only* logical ground. But it is plain that Causal Indeterminism may be founded logically on other grounds too. There would be no inconsistency in conceding the logical determinateness of events and states of affairs at the quantum mechanical level while at the same time denying that such events and states of affairs are related to each other by laws of invariable succession. And the latter denial would by itself, if it could be substantiated, demonstrate the falsity of the claims of Causal Determinism.

But can it be substantiated? It is sometimes claimed that even if we were able to perform precise measurements of both the conjugate parameters of an electron at a specific instant of time, it would be found that the precise state of the electron so defined is only haphazardly related to its future states. Imaginary experiments of this kind are envisaged by Heisenberg, Margenau, and Popper. Of such experiments Grünbaum[1] claims:

> The important point here is that such measurements, though entirely compatible operationally, are *not theoretically significant:* the specific

[1] op. cit., p. 714

> simultaneous values furnished by them cannot be regarded as defining a precise state of the atomic entity productive of information concerning its past and future behaviour in the manner of classical physics.

And he goes on to claim that if we do regard them as defining such a precise state, repeated runs of the same experiment will show that we were mistaken since the future values of its parameters would spread in accordance with the Uncertainty Principle. He concludes:[1]

> Thus, what is wrong with the kinds of simultaneous measurements we are considering is *not* that their joint execution and issuance in definite numerical values is operationally impossible; instead it is that the sharp values they do yield via *compatible* operations, when both are interpreted as exact values of the conjugate parameters of the theory, are at best theoretically sterile and at worst a source of false predictions. Accordingly, the Heisenberg principle must be understood as asserting that no *theoretically significant* exact measurements of conjugate values can be operationally compatible, i.e. simultaneously performable.

(where ' theoretically significant ' means ' productive of predictions '). Now it is clear that some such experiment as that here envisaged must be performable if the present claim is to be substantiated, i.e. if the supposedly irreducible statistical ' laws ' of quantum theory are not to be attributed merely to the indeterminateness of microphysical states but rather to the non-causal character of the relations between them. This is the only sort of empirical evidence which could establish Causal Indeterminism without involving the difficulties we have found in the thesis that quantum mechanical events are themselves indeterminate.

But even then it cannot establish it; for other difficulties too are involved. The first concerns the status of such imaginary experiments in quantum theory itself. As I said before, it is not all clear how, if at all, such experiments can be held to be operationally possible consistently with Heisenberg's Uncertainty Principle. For the Heisenberg Principle, as ordinarily understood, *does* rule out the possibility of making simultaneous precise measurements of conjugate parameters and not merely as Grünbaum would have us believe—the theoretical significance of such operations, i.e. their use for prediction. At least, if it does not preclude such measurements then all the interpretations, previously considered in (1), which purport to show that microphysical systems do not have determinate states, together with Bohr's claim that the renunciation of the Causal Principle follows from this alone, must be mistaken. While if it does—as is usually assumed—

[1] op. cit., p. 715

involve the impossibility of these measurements, then the argument in the present case collapses; for since it is then not the case that the state of a system can be determinately stated it is little wonder that no statements of its invariant relationship to other determinate states can be formulated. I cannot see any way out of this dilemma.

But suppose there is a way out. Even so, I want to argue, there are other difficulties which concern, not the status of this imaginary experiment *vis-à-vis* the rest of quantum theory, but its alleged indeterministic implications. Assume, then, the correctness of Grünbaum's claim that the Uncertainty Principle asserts only that exact simultaneous measurements of conjugate parameters are theoretically insignificant, not that they are operationally incompatible. Does this theoretical insignificance of such measurements—i.e. their uselessness for predicting—entail what he calls 'the Copenhagen view that the temporal evolution of the mechanical properties of individual atoms is irreducibly statistico-deterministic rather than deterministic'?[1] The argument that it does appears to go like this: it is claimed that even if we can determine the precise simultaneous values of the conjugate parameters of a single particle at two successive instants, we still cannot formulate causal laws of its development of the form. 'If p_1 and q_1 at t_1, then p_2 and q_2 at t_2'; for when we repeat our experiment we will find that the same initial state—p_1 and q_1 at t_1—is *not* always followed, after the same time interval, i.e. at t_2, by the state p_2 and q_2, but rather that the values of its parameter-variables ('p' and 'q') 'spread' in a statistical or probabilistic manner. It is, therefore, causally contingent whether the same initial state of a system—as determined by our measurements—will be followed by this, or that, or some other particular state of the system; but, at the same time, the aggregate of these state-sequences in repeated runs of the same experiment will be found to conform to certain statistically regular patterns. This is what Grünbaum means when he talks about the temporal evolution of the mechanical properties of individual atoms being 'irreducibly statistico-deterministic rather than deterministic'. Again, Lenzen claims that whereas 'in classical physics it was assumed that a statistical regularity is the manifestation of a more basic dynamical regularity . . . quantum theory has introduced statistical regularities which are not reducible, but fundamental'.[2]

We are now in a position to state more clearly what it is that is in dispute. We are considering the claim that there are performable

[1] op. cit., p. 720 [2] op. cit., p. 26

experiments in which the purely probabilistic, non-deterministic character of the formulae that describe the development of microphysical systems cannot be attributed to the indeterminateness of the states of those systems since their conjugate parameters for any given instant of time can be determined with complete precision—in a way which seems, to me at least, to be inconsistent with Heisenberg's Uncertainty Principle. The question is simply whether we must conclude from this that there is a real causal indeterminacy in the development of these microphysical systems, or, in other words, whether the probabilistic formulae or statistical regularities which quantum theorists use today are irreducible and fundamental, i.e. not capable of explanation in terms of a deterministic substratum of hidden variables. Like de Broglie, I believe that 'such an extrapolation does not appear in any way to be justified'.

What the argument overlooks is that neither the principle 'Same cause, same effect', nor the Causal Principle which involves it, can conclusively be refuted by evidence that two or more apparently identical systems do not have the same effects, i.e. do not develop in the same way. For it can always be argued—as was shown earlier—that if two such systems do *not* develop in the same way, then, despite appearances, their initial conditions could not have been, in all respects the same. Indeed, scientists often proceed as if 'having such and such an effect' were the very definition of 'having such and such a cause'. Arthur Pap, for instance, in his doctoral thesis *The A Priori in Physical Theory*[1] quotes Max Planck as writing: 'The state of a material system at a given instant is the function of all those variables the value of which at that instant completely determines the whole development of the processes which take place in the system.' Considered in this light, it is clear that in the present instance the very fact that the simultaneous exact measurements of the conjugate parameters of an electron do *not*, as is shown by repeated experiments, 'completely determine the whole development of the processes which take place in the system', can be taken to count against the proposition that these measurements adequately characterise the initial state of the electron. In other words, if the state p_1-and-q_1 at t_1 is not invariably followed at t_2 by the state p_2-and-q_2 (where the time-interval t_1-t_2 is invariant), then this will count as evidence for holding either that the values of 'p' and 'q' could not have been determined with complete precision after all, or that the state-description 'p_1 and q_1' is not a *complete*

[1] A. Pap, *The A Priori in Physical Theory*, New York, 1946, p. 65

state-description—even although completely sharp values have been given to its parameter-variables.

Now the second possibility here envisaged is identical with that postulated by Grünbaum when, in attempting to distinguish his own interactionist thesis from that of macrophysicalism, he claims that the particular pairs of attributes which furnish the mechanical state-descriptions of classical physics may not be the only attributes predicable of microphysical systems. (Since his own thesis is that the classically conceived parameters of physical systems are wholly interactional, he can avoid macrophysicalism, and so allow the autonomous existence of microphysical objects, only by supposing that such objects possess 'other' attributes which are not interactional, nor, therefore, classical). Thus, merely because a given system has been described with complete precision so far as its classical parameters are concerned, it does not follow that the state of the system has been completely described in respect of *all* its attributes: it may be that we need to take into account the values of other, non-classical or 'hidden', state-variables if we are to give a completely adequate description of the state of a microphysical system. Similarly, merely because two systems are alike in respect of their classical parameters it does not follow that they are completely alike, i.e. the same in respect of all their possible attributes: nor, therefore, does it follow that, if their future states are different, we must conclude that the principle 'Same cause, same effect' is false. For their initial states, as completely determined by both classical *and non-classical* parameters, may have been different also. There is no contradiction between 'Same cause, same effect' and 'Different cause, different effect'.

It is just this possibility of the existence of a deterministic substratum of 'hidden' variables which leads de Broglie to reject what he regards as the unjustified extrapolation from 'the uncertain and incomplete character of the knowledge that experiment at its present stage gives us' to the orthodox claim that this is 'the result of a real indeterminacy of the physical states and of their evolution'. For he continues;[1]

> It is possible that looking into the future to a deeper level of reality we will be able to interpret the laws of probability and quantum physics as being the statistical results of the development of completely determined values of variables which are at present hidden from us.

[1] op. cit., p. x

Again, Einstein adopts this point of view when he writes: ' Assuming the success of efforts to accomplish a complete physical description '—a description which, he claims, the statistical formulae of the present quantum theory do not provide—'the statistical quantum theory would, within the framework of future physics, take an approximately analogous position to the statistical mechanics within the framework of classical mechanics. I am ', he says, ' rather firmly convinced that the development of theoretical physics will be of this type; but the path will be lengthy and difficult.'[1] It is not without significance that David Bohm and others claim to have made some progress along this path.

But, it may here be objected, there is in quantum theory a logically rigorous proof—in the form of a theorem of von Neumann—that no such theory of a deterministic substratum is possible. This proof, as Scriven describes it, takes as its premises the statistical descriptions of contemporary quantum theory and then proves that ' it would be logically inconsistent to accept these, the most plausible descriptions of sub-atomic processes, and maintain that there exist some underlying deterministic variables of which the quantum theory variables are manifestations '.[2] Reichenbach, too, makes a similar claim for the absolute finality of the present-day quantum-mechanical descriptions. Thus his Synoptic Principle asserts that ' all observational knowledge ever attainable can be summarised in the form of a psi-function; in other words, that the description D of a physical state given by the psi-function is the most complete description possible '.[3]

Now it might appear to some that the von Neumann theorem offers us a purely *a priori* demonstration that a deterministic substratum cannot exist. But, as philosophers, we know that a purely *a priori* proof of the existence *or non-existence* of any matter of fact is impossible. The theorem, therefore, cannot be purely *a priori* after all but rather must include among its premises at least one contingent proposition. And clearly, if this is the case, then our rejection of the theorem's conclusion does not lead us to self-contradiction but only to the denial of a contingent proposition: that is, we can escape von Neumann's conclusion by denying the factual truth of one or more of his premises. And this means that the conclusion itself, viz. that the statistical descriptions of contemporary quantum theory are irreducibly indeterministic, takes on the status of a questionable and perhaps even

[1] op. cit., p. 672

[2] M. Scriven, ' The Present Status of Determinism in Physics ', *J. Phil.*, 1957, 54, 735

[3] op. cit., p. 214

arbitrary assumption. But worse is yet to come. For the theorem begs the question. As is clear from Scriven's account, the truth of the factual premises from which it starts, viz. the assumption that present-day quantum-mechanical descriptions are complete descriptions of what goes on in the microphysical world, is just what is in issue. Thus, we may conclude with David Bohm:[1]

> The conclusions concerning the need to give up the concepts of causality, continuity of motion, and the objective reality of individual micro-objects have been too hasty. For it is quite possible that while the quantum theory, and with it the indeterminacy principle, are valid to a very high degree of approximation in a certain domain, they both cease to have relevance in new domains below that in which the current theory is applicable. Thus the conclusion that there is no deeper level of causally determined motion is just a piece of circular reasoning, since it will follow only if we assume beforehand that no such level exists.

To sum up: It is quite evident that what physicists like Einstein, de Broglie and Bohm all object to, in their own way, is what I have called the ' epistemic-ontological confusion ' involved in any reasoning which regards the absence of an experience of causality as equivalent to an experience of the absence of causality. And what all the foregoing critique of quantum mechanics amounts to can be put quite simply by saying that the Causal Principle, i.e. Causal Determinism, cannot be falsified by any experience whatever since statements like ' There is no cause for this event ' (asserted by the ontological interpretation of quantum theory) can never be experimentally verified, while there is always the possibility (envisaged by the supporters of the epistemic interpretation) that such statements may be experimentally falsified in the microphysical world (e.g. by the discovery of a deterministic substratum of hidden variables which will account for both the apparently quite haphazard behaviour of individual objects at the quantum-mechanical level *and* the fact—inexplicable for the Copenhagen view—that these haphazard events combine into the statistically regular aggregates which comprise the deterministic laws of classical mechanics).

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[1] op. cit., p. 95

EMPIRICISM, SOLIPSISM, AND REALISM *

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Introduction

MUCH ambiguity in modern work on the relationship of consciousness and reality arises from the fact that the underlying assumptions are not always clearly stated and vary with the author, who may presuppose sense-data only, the existence of other conscious creatures, or that of a physical world, etc. The greatest difficulty of all however is caused by the uncertainty whether the author admits only such knowledge as is gained exclusively by our senses, or whether he presupposes other kinds of knowledge. Such extra-sensorial knowledge can be claimed to support any statement on any level, even the assertion that two concepts formed on different levels are identical.

In this paper, we start from two assumptions which are, in my opinion, the simplest and therefore the most general on which any view of the world can be based. On these, we proceed to build up a solipsistic world. We then add two more assumptions, which together we call the axiom of absolute symmetry. These are again the minimum and therefore the most general assumptions which express the belief in other persons and in a real world in a logically manifest form. We then investigate which changes the 'axiom of absolute symmetry' brings in. We shall also touch on the consequences of admitting extra-sensorial knowledge.

A *Absolute validity*

If anybody speaks at all, he assumes that there are correct and incorrect statements, and so do I. I have to apologise for making such vague and unproved assumptions, but I do believe that the proposition 'two two's are four' is true, and that the proposition 'two two's are five' is untrue. Here already, we part from Kant and his followers, who make this validity dependent on human thinking (i.e. on an abstraction made from a plurality of subjects) and call this an '*a priori* of human thought'. We disagree. To make logical truth thus dependent on other beings is a confusion of levels, which evidently rules out solipsism from the start.

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B *The empirical axiom*

The empirical axiom states that all knowledge, apart from that based on logical deduction, only comes to me through my senses. This axiom would say, for instance, that if I had never seen the sun and had never been told about it,[1] I could not know in which position it will be some time after dawn. I do not assert that this axiom, which rules out extra-sensorial knowledge, is true, but I shall assume, for the time being, that it is, in order to find its consequences. I shall return to this question at the end of this paper.

(1) *Solipsism.* The empirical axiom, if applied consistently and exclusively, leads straight to solipsism. This consequence has often been shunned, but it does follow nevertheless. If all my knowledge comes through my senses, so does my knowledge of other men, and 'other men' must be defined in terms of my sensorial perceptions only. The empirical axiom puts us into the dungeon of solipsism forever, and there is no way out.

A digression is necessary here. The solipsism of quality has always been recognised. Practically all thinkers agree that I can never find out what my neighbour experiences when he talks of 'red colour' or of 'fear'. It appears that there are some persons (Nelson?) who, never having felt fear, do not know what the word means. Now it has been said that relationships can be transmitted from person to person.[2] Ayer [3] rightly points out that what holds for qualities also holds for relations; but he does not follow this point up any further.

(2) The concept of existence, adding and cancelling of sense-data: If the empirical axiom imprisons us in the dungeon of solipsism for ever, this does not say that it prevents us from building up a world within the walls of this dungeon that completely corresponds to the world of common sense. What, in this world within the dungeon, does it mean to say that something exists? In the first place and fundamentally, it means that I see it; or, more generally, that I perceive it by one or more of my senses. The confirmation by more than one sense gives me 'greater certainty'.[4] Already at a second stage, the principle of economic thinking comes in. If I see the moon, I say that it exists. If I do not see it for a number of nights and then see it again, I may

[1] See below (B3) [2] See R. Carnap, *Unity of Science*
[3] A. J. Ayer, *Problem of Knowledge*, London, 1956, p. 235
[4] Cf. also Carnap, op. cit.

first assume that this is a ' new ' moon, but soon I shall say that it is the same. So far, the existence of the moon has been defined by pure observation only (observationally). But at the same time, we also define it ' operationally '[1]: To say that the moon exists, we must not only see it when we go to the window and look out by chance, but also when we go to the window in order to see it, provided there are no clouds, i.e. no plausible reasons why we cannot see it. As a further step, I say that something exists if I experience any effects caused by it, expectedly or unexpectedly. I say, for instance, that my landlord exists, not only when I meet him in the street (observationally), or because I know that I can see him if I go to his home at a time when he is there (operationally), but also because I get a letter from him if I do not pay my rent (causally). This aspect of existence leads us already to the use of the model in physics. If a group of phenomena always occur together or in certain definite—preferably quantitive—connections, we give this complex or whatever we suppose to ' cause ' it a name, and say that this thing ' exists '. A good example of this is electricity. The conviction that this concept ' actually exists ' becomes all the more certain if we can replace it by some imaginable model like ' current ' and then ' electron '. It becomes even more certain if expected or—even better—unexpected inferences which would follow from the model are actually observed. Similar considerations hold for the problem of existence of such concepts as momentum or energy that allow of no adequate or imaginable model. We assume certain quantitative connections, e.g. the conservation of energy, and believe in the existence of the concept if consequences of these relationships, expected and even better unexpected, turn out to be fulfilled.

So far, everything has been expressed in the vocabulary of everyday language, but all I have said stays within the confines of my personal experiences and therefore remains a group of pictures within the walls of my solipsist dungeon.[2]

Just as, for the sake of economy of thinking, I assume the right to add concepts such as electricity, not provided directly by my senses, so I also discard some things experienced by my senses, such as dreams and hallucinations, if they do not fit into my system of arranging my experiences and concepts.

[1] Cf. W. Russell Brain, ' Space and Sense Data ', this *Journal*, 1960, **11**, 187: ' Perception is from the first sensory-motor '

[2] Cf. also P. W. Bridgman, *The Logic of Modern Physics*, New York, 1960

It will be seen that the criteria which decide whether something can be assumed to exist are manifold, and each may be present in one or in many experiences or experiments referring to the same thing. Hence, on this level, existence and non-existence cannot be considered to be alternatives, but intermediate stages are possible. This is the reason why, when physics is built up on a purely observational and operational basis, controversies about the reality of concepts such as entropy or the electromagnetic field constantly arise. Actually, the criteria for existence on the sensorial side are completely different from those required by the theoretical physicist.

(3) The antagonism between egocentric perspective and interpersonal symmetry: It is necessary, on this level, to draw attention to two opposing tendencies, which we shall call on the one hand the egocentric or subjective perspective of the world[1] and on the other its interpersonal symmetry. The first is based firmly on definitions, the other is strongly suggested by the actual structure of the sense-data. They are best demonstrated with reference to Einstein's first paper on Special Relativity,[2] where both of them appear in a specialised form. It is well known to what extent philosophers were perturbed by Einstein's definition of simultaneity and its success in theoretical physics. It showed that it was erroneous to believe that the world could be described by several distant observers ' at once ', but that it could only be described from the standpoint or the perspective of one observer alone. Nevertheless, this fact had long been in easy reach of those philosophers who called themselves subjectivists. But its full structural implications do not appear to have been realised before. The general principle might be formulated as follows : I can only have such knowledge of the world as reaches me at my standpoint in space and time.

Einstein's definition of simultaneity is based on the idea—generally recognised by physicists—that to define a physical quantity one must state the way it is to be measured. Similarly, the principle that all knowledge of contingent facts is based on sense-impressions demands that, in order to define any concept referring to the world of my senses, I must state unambiguously the experiences on which this concept is based. Einstein gives his definition of simultaneity based on one observer only and on the system of co-ordinates moving with

[1] This corresponds to Bertrand Russell's ' Perspective . . . centred upon our own bodies ', but here the term is used in a much wider sense.

[2] Einstein, *Annalen der Physik*, 1905

him. He cannot base it on a plurality of observers and co-ordinate-systems at once, because this 'at once' has not yet been defined. Similarly, I must base the definitions of my concepts on my own sense-experiences only, because I do not yet know their relationship to the assumed sense-experiences of those phenomena which I call 'other men' or 'other animals'; I do not even know whether such sense-experiences exist. Both Einstein's and my own procedures are based on the fact that a description of the world which is founded on sense-experiences can primarily only be based on the experiences of one person alone and on what reaches his senses, because the relationship between the experiences of different persons must primarily be considered unknown.

But now Einstein goes on to bridge this gap between the various systems of co-ordinates by postulating that the laws of nature must be the same for all of them,[1] and, by applying the Lorentz transformations, he succeeds in translating the observations of one observer into those of another. This, of course, is a step of enormous importance. Without it, the observations of two observers in two different co-ordinate-systems, despite the laws of nature being the same, would be unconnected. Similarly, in defining my concepts in my world, I make use of the powers that I have assumed, namely to cancel experiences that do not fit my system (such as hallucinations) and to add others that I can imagine, but that have never occurred to me, and to construct my world in such a way that all laws hold equally for 'other living creatures' of which my senses give me evidence. Furthermore, I invent relationships between my sensorial experiences and those imagined ones of 'other living creatures', which make an unambiguous translation possible, corresponding to the Lorentz transformations. The fact that it is possible to find such laws and such translations—the fact that my sensorial experiences are such that this is possible—is the equivalent on this level of what common sense calls the real world in space and time.

These ideas may be illustrated by an example: I sometimes see a river, its banks, and ships travelling on it. I assume that other men also sometimes see the same, and that they see the river between its banks and the ships on the river, and not the other way round, i.e. that the general laws are the same. Furthermore, in a special case, supposing I stand on the North bank of a river flowing from East to

[1] It seems to me that he is perfectly successful only for Galilean systems of co-ordinates.

West, and I see a steamer coming down the river; I also see the shape of a man, standing on the South bank and looking North. Then I assume that he also sees the steamer coming down the river. But while my steamer moves from left to right and I see its starboard-side, his steamer moves from right to left, and he sees its port-side.

It must be stressed that the features of my sense-experiences which suggest symmetrical laws and rules of translation are very pronounced. I shall only give a few instances of this[1]: Once I have, as a child, learned to understand the spoken word, I find that the images I form from what other people tell me are, as a rule, confirmed by my own experiences, if I meet with a similar situation. They are not confirmed in every detail, but with sufficient accuracy to serve my needs. I can therefore use the images formed by what I have learned from other people in nearly the same way as I use my own actual experiences. Furthermore, there are certain details in my sense-experiences which, on the one hand, affect ' other creatures ' in a certain way and, on the other hand, affect my own consciousness as a whole, thus suggesting that my own consciousness is only part of an ' outer world ', as are also the bodies of ' other creatures ' that I see. For instance, if I see another man take a drug, I witness its effect on his behaviour. If I drink from the same bottle myself, I experience a change in my whole consciousness. Once I have become acquainted with all the causal connections that exist between details of my sense-experience, affecting my body and my consciousness (e.g. drugs, falling stones, etc.), I find that there are very few experiences (e.g. dreams) that I must cancel to form my symmetrical laws, though the number of imagined experiences that I must add always remains very great. But the rules by which I must add them become more and more fixed. All told, the circumstantial evidence that I and my consciousness are only part of a real world, the laws of which are symmetrical for myself and other creatures, is overwhelming. But it remains ' circumstantial evidence ' only. All I have is patterns of my sense-data, of their automatic changes and their responses to my acts of free will. All my concepts, observational and operational, are formed from them and my corresponding imaging. There is no outside standpoint, from which I could have a bird's-eye-view of the world. My description of the world must always start in the form of a traveller's guide-book, telling me what I see (observational), if I take a certain route (operational). If, from the experiences of my journeys, I can draw

[1] For a more detailed discussion, cf. Ayer, op. cit., p. 199.

a map, all the better. But there is no guarantee that such a map is possible.

C *The metaphysical axiom of absolute symmetry*

Now I smash the walls of my solipsist dungeon and postulate that there really is a world that is symmetrical with respect to myself and other conscious creatures, and that the rules of translation really hold. This axiom is doubtless metaphysical; we are at a loss if we are required to explain it. Yet all of us believe in it and postulate its absolute validity in the sense stated earlier in this paper. What is the position now? Though now the world is independent of the existence of every single one of us and symmetrical in its laws, we must still, if we want to define any concepts, base them on our sense-experiences, and hence the logical form of our definitions still remains egocentric. There are therefore two traces of solipsism in our knowledge: firstly the well-known fact that impressions such as colour cannot be transmitted from one person to another; secondly the fact that the construction of the definition of our concepts is egocentric.

Furthermore, one is led to the following conclusion. As, on the one hand, all our concepts are formed from our sense-data, our experience and our imaging only and, on the other hand, the validity of propositions which express relationships of these concepts is decided by sense-data, there must be a solipsistic proposition corresponding to each meaningful proposition in the real world. Consequently, to every world that the realist can construct, the solipsist can construct an isomorphous world, i.e. a world that contains corresponding elements and connections. However the opposite need not be true. Not all relationships that are possible in a solipsistic world are necessarily also possible in the world of the realist. The axiom of absolute symmetry brings in certain restrictions, which may cause difficulties. The most formidable of these arise from two sources. Firstly, the principle of symmetry of the solipsist is only founded on economy of thinking, and the solipsist can therefore waive it whenever this suits him. The realist however believes in his axiom of absolute symmetry. He will therefore have to find out all its consequences, face up to them and accept them. Secondly, the axiom of absolute symmetry identifies the ego, on whose perception and images according to the empirical axiom all concepts must be based, with any given individual in human society. This constitutes an identification of definiens and

definitum and a confusion of levels. Such a step may easily lead to paradoxes. In the following, we shall treat what appear to be the most important difficulties a realist has to face.

(1) Sensorial and inferred reality: We have noted that on the solipsistic level the criteria of existence are not uniform and can be based on a variety of circumstances, each of which can be experienced once or any number of times. On our present level, where we believe in a reality, it remains dubious what kind and what amount of circumstantial evidence is required in order to attribute to a concept the status of reality. The extremes, the 'existence' of sense-data at one end and the 'existence' of physical concepts such as entropy at the other end, differ widely, and it is not surprising that the identification of such concepts may lead to paradoxes. This difficulty can be partially overcome by introducing two concepts, those of 'sensorial' and 'inferred' reality. We are however still left in doubt as to what amount of evidence is required before we can apply the concept of inferred reality. But now, this question can be decided by personal judgment in each individual case and involves no fundamental logical problem. With respect to physics, Einstein, Podolski, and Rosen[1] have tried to solve this problem by giving the following definition: 'If, without in any way disturbing a system, we can predict with certainty (i.e. with probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity.'

(2) The ego as a specimen of *gens humana:* The axiom of absolute symmetry identifies the ego, on whose sense-data the structure of the world is based, with one certain individual out of many equals in the 'real world'. In a certain sense, the definiens thus becomes part of the definitum. For any individual with the whole of his mind is only part of the world. This implication of realism appears very controversial, but the following arguments can be advanced in its favour: Firstly, in mathematics the definiens is often found to re-occur as a special case or as a part in space of the definitum; e.g. if we define a power of x as x^n, where n can be any positive integer, x itself is a special case of the definitum. Secondly, a concept can be both defined and used without ever having been imagined or individually thought of by the person using it; e.g., if someone makes use of the number 'one million', he is presupposing and using the definitions of all numbers less than one million, the great majority of which have

[1] Einstein, Podolski, and Rosen, *Phys. Rev.*, 1935, 47, 777

individually never been in his mind. This shows that one cannot construct the paradox: ' The whole world is in the ego's mind; the ego is a person in the world and forms part of it.'[1]

(3) Imperfection of sense-data: There is a further difficulty which arises from the axiom of absolute interpersonal symmetry, because it places the ego and all other persons on the same level. The sense-data of different persons do not agree; consequently, though sensorial reality can be accorded to each, inferred reality can be accorded to none. The position gets even worse if one includes all conscious creatures, i.e. all animals, because some of these have sense-organs which work very differently from ours. This difficulty might be overcome if we define ' absolutely correct sense-data ' by extrapolation and accord inferred reality to things as given by these. But a fresh difficulty arises here: the sense-data of different conscious creatures differ not only in accuracy, but probably also in quality; thus in order to achieve absolutely correct sense-data, we must also overcome the difference in their quality by abstraction. These ideas have led to the distinction between primary and secondary properties of objects, the former being those which result from this abstraction.[2] But what remains of such sense-data after their quality has been removed by abstraction? Possibly only relationships. This would take us back on the realistic level to a view similar to Carnap's quoted above. This problem, which has been dealt with by Smythies and others,[3] shall not be treated here. But from what has been said, we can conclude that the empirical axiom in a realistic world is only compatible with a representative theory of perception.

We may mention here that the difficulty concerning perception does not arise from interpersonal symmetry alone, but only in conjunction with our assumption that the relationships inferred from our sense-experiences persist independently of the existence of conscious creatures, e.g. in an unpopulated world. If we consider this difficulty insurmountable and discard this assumption, the empirical axiom does not necessarily lead to solipsism, but it still permits of an idealistic view, similar to that of Kant, which makes the world dependent on the

[1] Note that we are not assuming that the ego *knows* that other persons have a mind. The question is merely whether the empirical axiom and the belief that other conscious creatures exist, are *logically* compatible.

[2] Exactly the same difficulty arises from considerations about the causal chain of physical and physiological processes which connect the outer world with the brain.

[3] e.g. J. R. Smythies, *Analysis of Perception*, London, 1956

existence of human beings. But an explanation is then required to account for the compatibility of the perceptions of different persons. This might take the form of a ' prestabilised harmony '.

(4) Imperfections of our brain: Conclusions similar to those in paragraph (3) seem inevitable if we compare the functioning of our own brain with that of other men's or animals' brains. None of these can be assumed to be perfect. Can an imperfect brain think absolute truth? If not, then we cannot think absolutely true propositions, and our axiom A does not hold.

(5) The replacement of sense-data by concepts that have inferred reality: The fact that our sense-data are inaccurate, and that only sensorial reality can be attributed to their quality, has led in some cases to their replacement by concepts constructed from other sense-data which are considered primary, inferred reality being accorded to these concepts. This procedure however may easily lead to confusion.

In his book *The Direction of Time*,[1] Reichenbach tries to replace the direction of time by entropy; this appears somewhat daring. On the other hand, no one objects to saying that sound is ' really ' a mechanical phenomenon and consists of waves in ponderable matter. What is the difference between these statements? It simply consists in the fact that our whole structure of physics can be built up without the concept of sound and could be understood by a person who has always been deaf, but it cannot be constructed without the experience of the direction of time. There is no objection to saying on the realistic level that we believe that waves are more real than our sensations of colour and sound, but this does not change the construction of our knowledge and our definitions, which remains isomorphous with that of the solipsistic world. The outcome of these examples is that we can cancel and consider as unreal any such sense-data that do not form an indispensable part of our logical structure of the world; but we cannot dispense with any such data which form an integral part of this structure. Nor can we, without risking paradoxes, identify them with concepts which are formed on a higher level.

If implied reality is denied to *all* sense-data, our view of the world is changed into a calculus which is unconnected with the world and the concepts of which have no definition at all in terms of sense-data; but this calculus may be consistent in itself. If then, by a further step, some of its concepts or numerical results are identified with certain sense-data or results of experiments, connection with the world is

[1] H. Reichenbach, *The Direction of Time*, Berkeley, 1956

re-established. By such a procedure, not all the concepts of the calculus must necessarily be defined in terms of sense-data or operations. This method has been adopted by some writers on quantum-theory and relativity.

(6) The use of experimental psychology in epistemological investigations: The question has often been raised whether and to what extent the results of experimental psychology or other sciences may be used in solving epistemological problems. From what has been said, it is clear that, if results of experimental psychology should be used to disprove solipsism, experiments on other persons must primarily be given a purely behaviourist interpretation. Otherwise, there would be a logical circle. Furthermore, in establishing the reality of the physical world, the use of results of the natural sciences, which presuppose the reality of this world, may also lead to logical circles. On the other hand, if the reality of the minds of other persons and of the world is assumed or has been established, there is no general objection to the use of experimental psychology or other sciences in the solution of epistemological problems. It must however be borne in mind that these sciences only transmit the knowledge of contingent facts.

(7) Complete description of the world: A complete description is possible neither of the solipsistic nor of the realistic world. In both cases, one comes up against the difficulty of complete self-description, whether the writer be the ego or a specimen of *gens humana*.

If we believe that any of the difficulties discussed under C (2), (3), and (4) cannot be overcome and constitute paradoxes, or if any other paradoxes can be deduced on similar lines, then our axioms (A), (B), and C are incompatible, and we are faced with the following choice: If we cancel axiom A and assume that it is impossible to make true statements, we are left in a position of such uncertainty that we cannot even legitimately state our own scepticism. If we cancel axiom C, we are solipsists. Cancellation of axiom B implies an extremely important consequence, i.e. that belief in the reality of other persons cannot be sustained without belief in extra-sensory knowledge. Realism is compatible with the empirical axiom only if these difficulties can be overcome.

D *Absolute knowledge*

It has so far been assumed that there is no knowlege about contingent facts except that which reaches us by means of our sensorial

perception. It is worth considering whether this assumption can be regarded as quite safe. There appears to be some evidence to the contrary. Firstly, there are the propositions, e.g. the theorems of mathematics, which Kant calls synthetic and *a priori*. To my knowledge, it has been neither proved nor disproved that propositions of this kind are based on sense-data only.[1] Secondly, there are some human beings who, at least at times, appear to possess the faculty of extra-sensorial perception. There are also the amazing experiments carried out with starlings that had been reared without ever having seen the sun, and that took their orientation from an artificial source of light, assuming that it moved round in the same rhythm as the sun moves in the sky.[2] If ' absolute knowledge ' is admitted, the difficulty arises that what one person claims to know by intuition or inspiration need not be acknowledged by others. The way out is to start from axioms on which all agree and, in case of doubt, to leave the decision to the experiment. This is what is actually done in science.

Einstein's first paper on special relativity showed that a generally accepted concept, the classical idea of simultaneity, was erroneous, and that the stage of a 'traveller's guide-book ' had not yet been overcome for the range of astronomical dimensions. Modern quantum-theory shows that this state still persists in the atomistic range. Therefore, even if there should be extra-sensorial perception, traces of solipsism still appear very markedly in the logical construction of physics. If one considers that the most fundamental concepts such as ' existence ', ' simultaneity ', ' ordinal number ' and the rigorous mathematical definition of ' limit ' have operational components, it appears doubtful whether it will ever be possible to describe the world in purely observational terms, i.e. to reach the ' map ' stage.

Conclusions

(1) If all knowledge is based on sense-experience, it follows that the world can primarily only be described in the form of a traveller's guide-book. If at length we succeed in piecing these descriptions together to form a map (symmetrical laws), this does not change the logical structure of definitions and of concepts of the edifice formed from them. If all synthetic knowledge only comes from sensorial

[1] I do not consider Kant's proof in his *Critique of Pure Reason* (Introduction) to be cogent.

[2] W.H. Thorpe, *Learning and Instinct in Animals*, London, 1956, p. 329; quoting K. Hoffmann, *Naturwissenschaften*, 1952–53, **40**, 148, 608

experience, then there is a meaningful law in the solipsistic world corresponding to every law in the real world.

(2) The axiom of absolute interpersonal symmetry, on which realism is based, introduces two main difficulties: The first of these is the identification of the ego with any specimen of *gens humana*, which constitutes an identification of the definiens with part of the definitum. This is discussed in (C2). The other difficulty arises from the fact that the realist must not give more significance to his own perceptions and thoughts than to those of other creatures. This has been discussed in (C3) and (C4).

If any of these logical problems should constitute a paradox, empiricism would be incompatible with realism, and a belief in the reality of other persons would then imply a belief in extra-sensory knowledge.

(3) There is no general objection to the use of experimental psychology and other natural sciences in epistemological research, provided it is realised that they transmit information on contingent facts only, and that logical circles are avoided.

Sense-data may be replaced by concepts formed on a higher level, but here too logical circles have to be avoided.

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THE STATISTICAL NATURE OF QUANTUM MECHANICS *

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I

THOUGH it deals ultimately with concrete realities of experience, quantum mechanics performs its calculations in an abstract Hilbert space of operators and vectors. The required connecting link between these two orders is furnished by the rule

$$[\mathrm{R}]_{\mathrm{av}} = \langle\phi|\mathrm{R}\phi\rangle, \tag{1}$$

which states that if we measure the quantity represented by operator R in the system described by vector $|\phi\rangle$, then the average or mean value obtained in the measurement will be given by the above mathematical expression. However one interprets the word 'average' —whether as the mean of measurements on a Gibbsian ensemble of identical systems, or as the mean value of measurements on the same system continually reprepared in its original state[1]—no way has ever been found of making physical sense of quantum-mechanical mathematics except in terms of a statistical description.

As a *physical* theory quantum mechanics can effectively be defined by the rule in equation (1), and this rule is essentially statistical. To bring out that statistical character most clearly we need examine only the measurement dispersion predicted by the rule. If we suppose that the vector $|\phi\rangle$ can be expanded in the eigenvectors of R as

$$|\phi\rangle = a_1|1\rangle + a_2|2\rangle, \tag{2}$$

where the respective eigenvalues are r_1 and r_2, then the rule in equation (1) yields for the dispersion ϵ^2, defined as

$$\begin{aligned}\epsilon^2 &\equiv [(\mathrm{R} - [\mathrm{R}]_{\mathrm{av}})^2]_{\mathrm{av}} \\ &= [\mathrm{R}^2]_{\mathrm{av}} - [\mathrm{R}]_{\mathrm{av}}^2,\end{aligned} \tag{3}$$

the value

$$\epsilon^2 = |a_1|^2\,|a_2|^2(r_1 - r_2)^2. \tag{4}$$

And in general this dispersion will not be zero, since it vanishes only for the very special case in which $|\phi\rangle$ itself is an eigenvector of R.

* Received 30.viii.61

[1] H. Margenau, *Phil. of Sc.*, 1937, 4, 337, Sec. 3

Consequently we may associate the statistical aspect of quantum mechanics with the measurements of nonvanishing dispersion that necessarily follow from the rule in equation (1). As a result, any doubts about the irreducibly statistical character of quantum mechanics must centre on the necessity of postulating that particular rule as the link between mathematics and experience. Dirac,[1] for example, chooses equation (1) on the basis of similarities between the mathematical properties of $\langle\phi|R|\phi\rangle$ and the well-known characteristics of mean values. But this argument, of course, is intended to show only the reasonableness of the rule and not to establish that it is an inescapable rule. The possibility may still lie open, therefore, of determining some different prescription for calculating measured values, and we must answer the question as to whether a new rule can be devised which will eliminate dispersion.

2

It is well known that von Neumann's work on quantum-mechanical measurement theory is of fundamental importance. But it is not so well known that von Neumann developed a general mathematical expression for the expectation value of a measurement which is independent of both the rule in equation (1), applicable to pure states, and the extension of that rule to mixtures:

$$[R]_{av} = \Sigma_n w_n \langle\phi_n| R|\phi_n\rangle, \qquad (5)$$

where w_n is the probability of the system being found in state ϕ_n. The expressions in equations (1) and (5) turn out, in fact, to be cases deducible from von Neumann's general formulation.

In order to establish the statistical character of quantum mechanics beyond question one must investigate the dispersion predicted by von Neumann's general measurement rule. This investigation was carried out by von Neumann himself in the course of elaborating his hidden-parameter proof, and the result is thoroughly conclusive: the mathematical structure of Hermitian operators in Hilbert space is incompatible with any but dispersive measurements.[2]

[1] P. A. M. Dirac, *Quantum Mechanics* (4th edition; Oxford, 1958), Sec. 12

[2] See J. von Neumann, *Mathematical Foundations of Quantum Mechanics* (Princeton, 1955), Chap. IV, Sec. 1 and 2. For a recasting of von Neumann's mathematics in the more familiar Dirac calculus, see J. Albertson, *Am. J. Phys.*, 1961, 29, 478–84. In the latter article some recent criticisms of von Neumann's work are examined.

Though the details of von Neumann's argument are sometimes complex, we can follow the main lines without difficulty and see that the rules in equations (1) and (5) are more than convenient postulates. They are, in one sense, the best that we can do in quantum mechanics.

In addition to the postulate that only Hermitian operators can represent physical quantities, von Neumann makes use of four others.[1] They are:

(i) If the quantity R is by nature non-negative, then $[R]_{av} \geq 0$.

(ii) If R, S, . . . are arbitrary physical quantities (Hermitian operators) and a, b, . . . real numbers , then $[aR + bS + \ldots]_{av} = a[R]_{av} + b[S]_{av} + \ldots$.

(iii) If a quantity is represented by R, then a function f of the quantity is represented by $f(R)$.

(iv) If certain quantities are represented by R, S, . . . , then the sum of these quantities is represented by $R + S + \ldots$, whether the quantities are simultaneously measurable or not.

On the basis of these assumptions von Neumann shows that the most general expression for the result of quantum-mechanical measurements is

$$[R]_{av} = \text{Trace } (UR) \qquad (6)$$

where U is a positive-definite Hermitian operator, characteristic of the ensemble under investigation, but otherwise undefined. 'Statistical operator of the ensemble' is the name von Neumann gives to U, and all information about the ensemble obtainable by measurement is synthesised in this operator. Thus far, we repeat, no explicit form has been given for U; only its existence has been proven. A choice is yet to be made from among the infinite number of positive-definite Hermitian operators available.

To make the proper selection von Neumann asks first if there is any feasible choice of U which will yield dispersion-free measurements. In an ensemble characterised by such a U, the dispersion ϵ^2 vanishes for *all* Hermitian operators R. From equation (3) we see that this means

$$\text{Trace } (UR^2) = [\text{Trace } (UR)]^2 . \qquad (7)$$

for all Hermitian R, and one draws the conclusion (a necessary but not sufficient condition, mathematically speaking) that the only U possessing this property is $U = 1$ or $U = 0$. But each of these possibilities is rejected by von Neumann—the first, because it becomes

[1] *Mathematical Foundations of Quantum Mechanics*, pp. 308–14.

impossible to define dispersion in such an ensemble; the second, because in this ' dispersion-free ' ensemble all physical quantities have vanishing expectation values.

By means of such general arguments von Neumann shows that in quantum mechanics there exist no dispersionless ensembles. And his argument is independent of the rule in equation (1). But if there are no dispersion-free ensembles it is still possible that there might be homogeneous ensembles, a type of ensemble which could be described as second-best, relative to the dispersion-free variety. In a homogeneous ensemble any given physical quantity has the same expectation value whether measured in the whole ensemble or in any possible (non-null) subdivision of it. Dispersion will exist, but both the dispersion and the expectation value will be the same throughout all sub-ensembles.

As the second stage of his argument von Neumann proves that homogeneous ensembles do exist, and that the necessary and sufficient condition for an ensemble to be homogeneous is that its statistical operator U be a projection operator such as $|\phi\rangle\langle\phi|$. For a statistical operator of this type von Neumann's general rule reduces to the rule in equation (1). A homogeneous ensemble, therefore, is identical with the traditional quantum-mechanical state.

3

On the basis of von Neumann's work we can rest two conclusions about the statistical nature of quantum mechanics. The first is that a mathematical theory of Hermitian operators in Hilbert space cannot be reconciled with dispersion-free measurements. And the second is that the customary way of calculating measured values in quantum mechanics (equation (1)) is actually the best rule available—best in the sense that though it does not eliminate dispersion it does describe the results of measurements on homogeneous ensembles.

Von Neumann's analysis of dispersion, we must note, has not exploited the peculiar properties of non-commuting operators. His result is thus independent of and additional to the so-called uncertainty principle. The extreme generality of von Neumann's analysis—limited only by the broad postulates listed above—means that no form of deterministic or non-statistical quantum mechanics is possible. This conclusion he drew in his hidden-parameter proof.

The way still lies open for constructing a thoroughly new theory

not based on the mathematics of Hermitian operators in Hilbert space. To some order of approximation a theory of this type would have to yield the experimentally confirmed results of present-day quantum mechanics, but whether such a theory can be elaborated remains to be seen.

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REVIEWS

ON WHAT THERE IS IN PHYSICS

THERE is a region between epistemology and theoretical science which has been somewhat undercultivated by philosophers of science, and which contains a number of problems which scientists pass to philosophers and philosophers disown as the primary concern of science. Mr Harré describes his entry into this region [1] as a ' study in prescriptive metaphysics ', and explains this as a discussion about the existence presuppositions of actual, possible, or desirable scientific conceptual schemes. His main concern is to describe two of these, which he calls *positivism* and the doctrine of *ontological depth*, and to argue that although positivism cannot be definitely refuted, yet it can be undermined by showing that it implies consequences for scientific theory and practice which do not correspond with what science is usually taken to be about. The main argument of this type is drawn from the possibility of extending by ' ontological experiments ' the domain of things which are said to ' exist ', for such extension is contrary to the positivist assertion that there exists only *one* ontological kind, whether this be interpreted as sense-data, pointer-readings, operations, physical objects, or what-have-you. There are also several subsidiary arguments against positivism as expressed in various forms. One of these is an attempt to show that theories constructed strictly in accordance with positivist criteria cannot make use of heuristic models, and that models (including models disguised in metaphorical use of scientific language) are essential for the predictive character of a theory. This is a theme I have discussed in my ' Theories, Dictionaries, and Observation ', and so I shall not pursue it here.[2] Another undermining operation, forming the last section of the book, is a brief attack on the Copenhagen interpretation of quantum physics, including a much needed but too brief analysis of the phrase ' complementary descriptions ' and all that the Copenhagen adherents attempt to do with that obscure notion.

The most interesting problem in the book is however that of existence-claims, and here Mr Harré strikes out into deep waters and uncharted seas. He points out that any general epistemological discussion of ' what there is ', or of whether all things of a certain kind (theoretical entities, or physical objects, or sense-data) ' exist ', is useless in the philosophy of science, because it always leads to all-or-none solutions, whereas what are left unelucidated are the kinds of distinctions we still wish to make between, say, okapis and chimeras, or between caloric, neutrinos, and magnetic fields.

[1] R. Harré, *Theories and Things*, Sheed & Ward, London, 1961. Pp. 114. 5s.
[2] This *Journal*, 1958, **9**, 128

While not going so far as to agree that such discussion is useless (and as we shall see, Harré himself indulges in some of it), it is certainly true that even the best and most detailed examples of it treat the problem of entities in science as a trivial special case, and ignore entirely the internal scientific distinctions which should specially concern the philosopher of science.[1] The latter discussion is indeed so new that there is no established terminology for it, and Mr Harré's text abounds with objects, pseudo-objects, entities, domains, universes of discourse, ontological classes or kinds, models, parent-situations, bridge-statements, formal models, candidates-for-reality, paradigm-case-procedures and paradigm-case-arguments, family continuities, and ontological experiments, all inevitably used in philosophically novel ways and not all of them doing the work they are paid to do by the definitions sometimes over-elaborately given to them. (There is also something called an 'indentification', but not even its occurrence in three different places was able to persuade me that this was not a misprint.) However, having got over the reviewer's grumble about the forms in which Mr Harré sometimes expresses himself, I come to the matter of his arguments.

These are, broadly, to determine what are the criteria for existence of scientific entities, and to show that positivism gives too narrow an account of these. In the first main section, entitled *Models to Mechanisms*, Harré distinguishes the programme of ontological depth as that of showing 'how nature really works' in terms of *mechanisms*, from the programme of positivism, which is concerned merely with concomitances of observables. He argues that full scientific explanation involves first the postulation of hypothetical mechanisms, or *models*, which are then examined to see whether they are indeed the mechanisms underlying observables. He takes it as proved that there are descending orders of mechanisms that have been discovered in science: physical objects—molecules, or organisms—cells—constituents of cells; and concludes that there are three possible fates in store for a model:

(i) It may be rejected as a real mechanism, either because it could not logically be one, or because it has been empirically refuted.

(ii) It may be a 'candidate for reality' still under examination.

(iii) It may be a proved or established mechanism.

In case (i), the model may continue to be used in connection with a theory, but this can only be for reasons of convenience, as with the idea of heat as a fluid flowing, and the model will carry a 'disclaimer of reality'. In cases (ii) and (iii), existence-claims for the model are relevant, and are settled by what Harré calls *ontological experiments*.

[1] Principally, W. V. Quine, 'On what there is', *Rev. Metaphysics*, 1948, **2**, 21, and R. Carnap: 'Empiricism, Semantics, and Ontology', *Rev. Int. de Phil.*, 1950, **11**, 20. An exception to this general neglect is to be found in the important paper by P. K. Feyerabend: 'An Attempt at a Realistic Interpretation of Experience', *Proc. Arist. Soc.*, 1957-8, **58**, 143

Before considering these existence-claims, something should be said about Harré's three kinds of model. First a small caveat. It is not clear what if anything is added by talking of ' reality ' here to the usual way of expressing the matter, which is to say that theories may be

(i) Either false, or neither true nor false.

(ii) Possibly true and possibly false.

(iii) True.

I do not think Harré does mean more than this, for he later remarks of a particular theory ' this theory has been neither confirmed nor shown to be false. That is as much as to say that the model . . . is still a candidate for reality ' (p. 29). Talk of ' mechanisms ' here also somewhat confuses the issue, because it adds to a thoroughly innocuous and acceptable distinction overtones of what sort of thing a true theory is, and on this, as we shall see, Harré is not so uncontroversial.

Even here, of course, what Harré says would not be taken to be innocuous and uncontroversial by all philosophers of science. For he boldly asserts, *pace* Popper, that there are true theories, that ' complete and final verification is possible, at each descending order of mechanism ' (p. 27). Theories may come and go, but we are quite sure we have shown that there *are* bacteria, whatever theory of the operation of our microscopes is in vogue at the moment. Here Harré is surely perfectly right, and is pointing to a feature of scientific discovery which the inductivists take seriously but fail to elucidate, while even Popper allows that the domain of what we conventionally call ' fact ' is extended by scientific discovery.

The essential point about Harré's classification of models, however, is that models in category (i) are not discarded, but that we wish to distinguish them from mechanisms. This leads him to the following argument against positivism: According to positivism, no models are the kinds of things that are true or false, provable or refutable, since only statements about observables have these characteristics. But we do do experiments whose outcome is regarded as showing that the model is the true mechanism of nature, and distinguish this case from that in which the model is merely heuristic and neither true nor false. Hence positivism does not adequately describe the practice of science.

The next three sections are central to the book, and pursue the question of how the decision implicit in model type (iii) is made. Harré's key notion here is the important and interesting one of *family continuity*. This is defined in terms of a sequence of events, like a matrilineal genealogy, which is linearly ordered and in which one member partially, and only partially, overlaps each of those adjacent to it. There is optical family continuity between, for example, an okapi and a virus, because there is a sequence of objects—okapis, drops of blood, red corpuscles . . . viruses—such that any two adjacent ones can be ' seen ' in the same sense of ' see '—with the naked eye,

with a magnifying glass, with a light microscope, with an electron microscope—and in the case of each object, it is recognisably the same object in at least two of these methods of 'seeing'. Family continuities can be similarly defined in terms of any of the other senses, and hence are not supposed to impute any priority to 'seeing'. Harré now asserts that if any member of a family continuity exists, any other member of it exists, and is of the same ontological class. This establishes the existence of, for example, viruses in family continuity with okapis, but, he argues in various places, it fails to establish the existence of electrons, or potential energy, or magnetic fields, or wave functions, or ideal gases, or the unconscious.

The criterion of family continuity has the merit that it does not presuppose any particular view about the ontological nature of the objects involved. They may all be sense-data, or operations, or physical objects, depending on our all-or-none epistemology. What the criterion does do is to place objects in that ontological class to which what are ordinarily called physical objects belong, and it shows that new objects can be placed in this class by means of certain appropriate ontological experiments (namely, the sequence of observations with magnifying glass to electron microscope). Such experiments do not find objects of new ontological kinds (do not extend family continuity *across* ontological classes, as Harré puts it). But just because they do not do this, it is not clear that they undermine all types of positivism. For if positivism is the assertion that only *observables* exist, where what can be 'observed' only with technical aids like microscopes is ruled out *a priori*, then this doctrine is undermined, but it is a very implausible form of positivism, and one which would presumably even now have to rule out the back of the moon from the class of existents. But if positivism is the much more plausible assertion that only one ontological kind exists, then the family continuity criterion *defines* such a kind, and hence cannot touch the positivist assertion.

Another doubt about the criterion arises when it is examined from the point of view of what it rules out. Harré sees that the things which it rules out cannot all be treated in the same way, for some may be of a different ontological kind and may be ruled *in* by a different type of ontological experiment; some may not exist in fact; and some may not be logically the sorts of things which could ever be said to exist. The case which affects the status of the criterion most critically is that of the electron. *Prima facie* this looks like an entity which is the end-product of the continuity-sequence physical object—molecule—atom—electron, but Harré finds that there is *no* such family continuity (associated with any sense-organ) into which it fits. This seems very queer, for if it is true, it is difficult to see how there is *any* connection between electrons and observables, and if this is the case, what are electrons doing as acceptable theoretical entities in science at all? Harré's reply is that *disturbances* which are said in the electron-model to be *effects*

of the electron are observed, but not the *thing* which is said to cause the disturbance. Now this may be taken to mean that an electron is not a *thing* (not in the same ontological class as a physical object), and in that case its existence might be dealt with along the same lines as that of a magnetic field, which Harré discusses later. But meanwhile the alleged failure of family continuity here has not been made out. What is this distinction between the *effects* of a thing and the thing itself? Might it not be said equally that the electron micrograph shows the *effects* of a virus, namely its deflections of fast electrons? And is it after all true that an electron cannot be put into a family continuity? Take any of the experiments in which we do say we detect electrons, and work up to ordinary physical objects through the appropriate sequence of experiments—for example, an electron produces a scintillation on a fluorescent screen, so does an electrified pith-ball, and we do other experiments with pith-balls to assure ourselves of their existence and of their electrification. It is no reply to say that the 'observings' must all be of the same kind, and are not of the same kind in my example, because they are not of the same kind in the microscope example either. There they are certainly all 'seeings' of something the shape of the object, but they are not 'seeings' of the same *kind*, if by this is meant by the same physical mechanism. If the mechanism can change from naked eye to electron microscope, why not from fluorescent screen to electroscope? And why should continuity of *shape* be uniquely important here? A ghost has continuity of shape with its predecessor in the flesh, but we have grave doubts whether it is an object of the same kind as a human being, or whether it is being 'seen' in the same way. In spite of his attempt to extend his criterion to other than the sense of sight, Harré has been misled by an archetype of 'object' in which the surest way to know whether something is there is to look and see, and because the electron does not fit into *this* family he denies its existence in the continuity sense.

But have we not now proved too much? For we seem to have shown that if something has observable *effects*, then it can always be put into some family continuity such as Harré describes, and hence, according to him, it will be of the same ontological kind as ordinary objects. Thus *any* theoretical entity in an acceptable theory 'exists' in the same sense as chairs and tables, including magnetic fields (linked by family continuity with a tense rope), wave-functions, and the unconscious, because if they could not be detected by some effects, they would not be present in the theory at all. But this is a total confusion of categories. The error lies, perhaps, in the assumption that members of a family continuity *are* all of the same ontological kind (compare the ghost example), or perhaps the notion of family continuity needs tightening up in such a way that it admits only what we are disposed to call *objects* ('individuals', 'substances') and hence to make some of the needed distinctions between *things*, like molecules, bacteria, and

viruses, on the one hand, and magnetic fields, energy, charge, whose thi like or predicate-like status is still obscure. Does family continuity instance necessarily involve an ordering of size? Some of Harré's rema about ' magnification ', ' fine-grain experiments ', and telescopes, lead to think so, but then what becomes of family continuities of the ot senses? These questions need answering, because I remain convinced t in pointing to the notion of family continuity Harré has got hold of sor thing important, but its development is at present far from complete.

In addition to family continuity, Harré suggests two other criteria existence which admit some things to our ontology which are not admit by continuity, and a corresponding type of ontological experiment by wh fulfilment of these criteria is tested. The criteria are (1) spatio-tempc location and (2) fulfilment of our stable expectations for things of the ki in question. (1) alone is clearly not sufficient, since, for instance, mira satisfy it, but do not satisfy (2). Magnetic fields, on the other hand, sati (2) and hence, in a derivative sense, (1), since it is our expectations of th which can be spatio-temporally determined. Harré argues that fields m be said to exist in this, different, sense, but should not be assimilated to same ontological class as objects, because they are not members of the sa family continuity. Electrons, again, are said to fail to pass this test, on grounds that they are only dubiously space-time locatable and fail to cc form to our expectations. This is surely a mistake. Electrons certai fail to conform to our expectations for classical particles, but this is only say that billiard-ball electrons do not exist because the theory which scribed them was *false*. Electrons as accepted in physical theories today conform to our expectations for electrons as described in those theorie see any TV screen for confirmation.

It may well be, however, that electrons ought to be regarded as mc like magnetic fields than like okapis, and this is a point which Harré touc upon in various references to ' objects ' or ' individuals ' as an ontologi class. It does indeed sometimes appear as if he equates the problem ' what exists ' with the problem of what are *objects*, and this is of course in fi traditional style, because properties and relations are *classes* and classes only held to exist in the most liberal of ontologies. This style, howev goes ill with the discussion of magnetic fields, energy, and electrons, whi may be said to exist by criterion (2), but are held by more traditional c tologists to be *predicates*. And Harré himself explicitly denies that field i substance, on the grounds, apparently, that the nineteenth-century aetl theory is false. Obviously a lot of spade-work needs to be done here, a I shall only offer a few remarks suggested by Harré's discussion.

Harré has several sets of criteria for ' objects ' or ' substance ', some which overlap his criteria for existence. The first set is suggested Strawson's definition of an individual as that which can be identifi

re-identified, and discriminated from other members of its domain. Now it is clear that if ' existence ' is to be restricted to such objects, various theoretical ' entities ' of physics will fail to conform, not for any of Harré's reasons, but because they are not like physical objects in the respects described by Strawson. Neither energy nor electrons can be re-identified or discriminated from other members of their domains, for reasons which are different in the two cases, and are concerned with their physical characteristics as described in physical theories. And yet energy was plausibly regarded as a substance in the nineteenth century, and many physicists would still wish to say that electrons are object-like. I shall return to this later. Harré's second suggestion on substance is that his criteria (1) and (2) for existence are criteria for substance ' in ordinary cases ', for instance for okapis and viruses. But he clearly thinks that in the second criterion he has something which allows existence to be predicated of that which is not substance in this sense, for example magnetic fields. Thirdly, he gives as criteria for the substantial nature of molecules, that they are cohesive and extensive, and uses this to deny, as a matter of *logic*, that ideal gases are substantive, or that they could ever have been candidates for reality.

There are several strands here that need to be disentangled.

1. There is first the distinction between *true* and *false* theories, which Harré elucidates by means of his three kinds of model. In this sense, any theoretical *entity* can be said to exist if it appears in an accepted theory (Quine: to be an entity is to be a value of one of our variables which makes some of our statements true). Since, however, we are not concerned with the logician's problem about the existence of *classes* it is logically illiterate to speak about the existence of theoretical *predicates* when we are referring either to a problem about what is to be considered an entity and what a predicate, or to the acceptability of such predicates in a true theory. What corresponds for a theoretical predicate to the existence of a theoretical entity is surely something like this: a theoretical predicate has ontological (as opposed to heuristic, convenient, etc.) significance when it is predicated truly of an existing theoretical entity. The criteria for existence in *this* sense will be something like Harré's criteria (1) and (2), and they will be tested by the kind of ontological experiment which he describes as ' reductive ', whose success places a model in category (iii) as a proved mechanism. Here, as he points out (p. 69), the mechanism may be of a different ontological kind from the phenomena—for example, the electromagnetic theory may explain reductively the structure of cells, but is of different ontological kind from these. Thus mechanisms are not necessarily those of classical mechanics.

2. This brings us to the second problem, namely what are to be counted as theoretical *entities* and what theoretical *predicates*, and this is closely related to the assertion that there are different *ontological kinds* and to the various

characterisations of *substance*. Of all the problems about existence that Harré touches on, this seems to me to be philosophically the most interesting.

The history of the notion of substance has been closely bound up with the scientific question of what are the essential, or primary qualities of matter. In doing philosophy of science we must beware here, as well as in the debate on existence, of being stampeded by epistemologists into all-or-none solutions. We all know how Berkeley emptied the problem of substance of scientific significance, and how a resulting ambiguity infects all interpretations of Kant—does the statement, for example, of conservation of substance have any *synthetic* content, or is it tenable whatever the empirical facts are found to be? But, as with the question of existence, when we turn round for a moment from the epistemological frontier, we are liable to find scientists still arguing about something which *looks like* an earlier and discarded stage of the epistemological discussion, *but is not*, because their answers *do* have scientific consequences. *This* is surely the proper area for the philosopher of science in his investigation of the problem of substance.

An illuminating case-history here is the nineteenth-century discussion of the 'reality' of *fields* as 'physically continuous media'. At least five criteria for reality were suggested:

(*a*) A postulated field is real if it has more than one detectable property, or if more than one kind of observable correlation can be derived from it. This is to avoid the charge that a field is an *ad hoc* construction from one kind of observation, and ensures that it has a proper place in a scientific theory if only on grounds of economy of description. It appears to be a criterion for existence in the sense we have just described in paragraph (1) above, and it would apply equally to theoretical entities and theoretical predicates.

(*b*) Propagation of action through the field is affected by *material* changes in the region of the field. This was one of the criteria which led Faraday to ascribe 'reality' to the electric and magnetic fields, for it seems to show that 'something is going on' in the intervening space, and not merely at the sending and receiving ends where there is observable matter. This is a criterion for the presence of *substance* in the intervening space, and requires more than the first criterion, for that might be interpreted only as saying that the field is a predicate or set of predicates of observable matter.

(*c*) A mechanism (in the sense of classical mechanics) can be imagined to account for the propagation of action through the field. This criterion was left unsatisfied when the search for a mechanical aether was abandoned, but it should be noticed that the criterion was not regarded as *necessary* for the reality of the field, because its non-fulfilment did not close the debate.

(*d*) Propagation of action through the field takes time. This, like (*b*), was taken to indicate 'something going on', for it was inconceivable (as Maxwell put it) that there should be propagation in time 'except either as

the flight of a material substance through space, or as the propagation of a condition of motion or stress in the medium already existing in space '.

(*e*) The presence of energy in the field. In virtue of its conservation, energy already has one of the characteristics of ' substance ', and so the reality of the field tends to become *equated with* the presence of energy. The conservation of energy in fact, together with satisfaction of (*d*) (which is an experimental question), *entails* satisfaction of (*e*), because unless energy is located in the field during the time which elapses between its emission from the source of an action and its reception at some point distant from the source, energy is not conserved during that time-interval. Thus (*d*) together with (*e*) came to be decisive in the general view, still on the whole shared by physicists, that reality has to be ascribed to the field.[1]

This kind of discussion is liable to sound very queer to the epistemologist. Is not energy, he may ask, a measurable *property* of bodies? How then can it be a *substance?* But this is a misunderstanding. Substance, in science, is always identified as something having properties of a certain sort—thus Cartesian substance has (or is) extension and motion; the substance of atomic philosophies has inertia, impenetrability, extension, and motion; Newton's substance has in addition attractive and repulsive powers; and the substance of relativistic electromagnetic theory has mass-energy, charge, and spin. That is to say, each fundamental physical theory describes its entities in terms of a constellation of ' primary ' qualities, which are primary in the sense that all other qualities predicated in the theory can be reduced to them, and which are usually also distinguished from secondary qualities by being *conserved* according to laws stated in the theory. Thus in Cartesian physics there is conservation of motion; in Newtonian physics conservation of mass and of momentum; and in electrodynamics conservation of mass-energy, charge, and spin. When, therefore, it is asserted that energy *is* a substance, this is an elliptic way of saying that if a quantity of energy is located at a given spatio-temporal point, then this, for the electromagnetic theory, is sufficient for the assertion of substance at that point, for the kinds of reasons suggested by the criteria listed above. In Newtonian theory on the other hand, energy is a secondary quality defined in terms of primary qualities by the expression mv^2, and the assertion that this has a value at a given spatio-temporal point implies that there is mechanical *matter* present, since energy cannot be defined at points where there is no matter.

All this implies that each physical theory determines its own kind of substance. It determines correspondingly what it wishes to call ' entities ' or ' objects ', as more or less spatio-temporally continuous collections of properties which are more or less invariant. The vagueness of this descrip-

[1] For more detailed discussion and references, see my *Forces and Fields*, London, 1961, pp. 195-225.

tion of what it is to be an entity matches the lack of definiteness in physics itself about what is to go into this class. For Descartes discriminable objects are somehow 'pieces of extension' moving together (whatever he can have meant by this); for classical mechanics it is mass-particles; and for modern physics it is, doubtfully, fundamental particles, which are identifiable, but neither re-identifiable nor discriminable from their like; which are discontinuous in space and time both on the large and on the small scale, because they may be created and annihilated and have no continuous spatio-temporal path between creation and annihilation; and which are only just admitted as entities because of short-term invariance of rest-mass, charge, and spin.

It is idle to hope for any light upon *this* debate from an epistemological definition such as that an entity is one of the values over which variables of the theory range, because it is precisely what this domain of values is that is often a matter of dispute within physics. The question of whether it included fields has been a controversial one in the past; and the question of whether it includes electrons is at present a tricky one, whose solution is partly technical, requiring a careful definition of 'electron' in relation to the 'states' which are the actual values of variables in an axiomatised quantum mechanics such as Von Neumann's, but which is not wholly technical, because it may be said that such an axiomatisation does not do full justice to the substance-like *models* which still have a place in physics, and in any case the question of whether there can be 'hidden variables' whose values are sub-quantum particles is still a matter of debate. In fact the act of axiomatising a physical theory in order to answer the question 'What are the values of its variables?' itself implies that a certain interpretation of the theory is adopted, and its adoption is equivalent to the decisions involved in answering 'What are entities?'

3. Finally, there is the question of 'ideal' as opposed to real entities, and what Harré calls 'formal' or mathematical models, which he says are always in category (i) of models because they *logically* cannot be the sorts of things which exist.

Harré repudiates ideal gases from the category of things which could logically exist by requiring of *substance* that it should be cohesive and extended. But if the foregoing argument is correct, it follows only that ideal gases are not substances *in the sense of classical physics*. There is nothing *logically* to prevent the existence of ideal gases, and if they were found to exist (that is, to be acceptable theoretical entities), that would indicate that the definition of substance in classical physics is too narrow. Ideal gases do not in fact exist, but this is to say that the theory which contains them has been refuted (by deviations of actual gases from Boyle's law, etc.). Thus they do not form a logically distinct category from that of entities asserted in *false* theories.

The case of formal models is somewhat different. If these are purely mathematical models unconnected with any empirical interpretation, then certainly they do not describe the kind of things which exist, since they are mere formal schemes describing nothing at all. If, on the other hand, they are mathematical models such as the four-dimensional curved space of relativity, then they are acceptable theoretical concepts with some indirect empirical interpretation, and can surely be said to exist in the sense of being described in a true theory. The only way in which this could be denied, it seems to me, is again by appealing to a too-narrow conception of substance, and our previous objections to this hold in this case also.

All these matters are so full of pitfalls that I am uneasily aware that in the attempt to interpret and analyse I may have missed some significant factors in Mr Harré's argument. This review is already too long, and I can only close it by saying that his boldness pays off, even at the cost of some logical looseness, and that I hope his excursion into ontology will stimulate further pioneer discussions of this important and neglected topic in the philosophy of science.

MARY HESSE

A Critical Examination of the Belief in a Life After Death. By C. J. Ducasse. Blackwell Scientific Publications, 1961. Pp. xvii+318. 70s. 6d.

THROUGHOUT all crises and developments in human society, throughout war, revolution, and the exploration of outer space, the question of whether the personality survives death remains perennially one of the most significant issues in religion and philosophy. Hence we are fortunate that Curt J. Ducasse, Professor Emeritus of Philosophy at Brown University, has in his latest book given an able and wide-ranging analysis of arguments and evidence for and against the reality of life beyond the grave.

He makes clear at the outset that he is not talking about a *post mortem* existence based on the orthodox Christian doctrine of a literal resurrection of the body, but is discussing survival in psychological terms as meaning a continuance of the human 'center of consciousness' with its memory and awareness of self-identity essentially intact. He states that the New Testament stories about the appearances of Jesus after his crucifixion 'make sense only if interpreted as reports of what are commonly called "apparitions" or "phantasms" of the dead'.

The author also eliminates the idea of *immortality* from his study:

> For immortality, strictly speaking, is incapacity to die, which, as ascribed to a human consciousness, entails survival of it *forever* after bodily death. But survival for some indeterminate though considerable period, rather than specifically forever, is probably what most persons actually have in mind when they think of a life after death. Assurance of survival for a thousand years or even a hundred, would for those of us who desire survival, have virtually as much present psychological value as would assurance of survival forever (p. 6).

Professor Ducasse takes the position that 'neither religion nor theology really provides any evidence that there is for man a life after death'. But his philosophic analysis leaves the door open for evidence and proof of *discarnate* survival through various types of paranormal phenomena, including those brought to light by hypnosis and psychical research, and those offered by mediums as messages from the dead or materialisations of their spirits. He has obtained first-hand information about Spiritualism not only by carefully studying the vast literature of this religion, but also by attending mediumistic seances.

At one of these, described in a chapter on 'Occurrences *Prima Facie* Indicative of Survival', he tells how

> some eighteen fully material forms—some male, some female, some tall and some short, and sometimes two together—came out of and returned to the curtained cabinet I had inspected beforehand, in which a medium sat, and to which I had found no avenue of surreptitious access. These material forms were apparently recognized as those of a deceased father, mother, or other relative by one or another of the fourteen or fifteen persons present, and some touching scenes occurred, in which the form of the deceased spoke with and caressed the living. One of those forms called my name and when I went up to her and asked who she was, she answered 'Mother'.

But, alas, she did not, according to Ducasse, 'in the least resemble' his mother! (p. 167).

The author, troubled over the old adage, 'If ghosts have clothes, then clothes have ghosts', makes the following comment:

> If one assumes that the clothing the apparition or materialization wears is materialization only of a memory image of the deceased's clothing, then would not consistency dictate the conclusion that the now temporarily perceptible parts of the deceased's body are materializations likewise only of a memory image of his appearance and behavior? (p. 169).

After considering the hypothesis that telepathy could explain the extraordinary communications offered by mediums which apparently divulge information known only to a dead person or somebody acquainted with him whom the medium has not known, Ducasse claims that the quality, quantity, and diversity of evidence 'we get over the mediumistic telephone' dictates the conclusion that 'the balance of the evidence so far obtained is on the side of the reality of survival'.

The author devotes a whole chapter to criticism of scientists in general, the majority of whom, he declares, are ready to dismiss without investigation, on *a priori* grounds, the objective existence or evidential value of paranormal phenomena such as the seance described above and many other occurrences. He quotes with approval as applying today the words of Professor James H. Hyslop, founder of the American Society for Psychical Research, in his book *Contact with the Other World*, published in 1919:

> Science, content, without thorough inquiry, to confine its investigations to the physical world in which it has achieved so much, will not open its eyes to anomalies in the realm of mind and nature and so degenerates into a dogmatism exactly like that of theology (pp. 145-6).

Yet for the last half century neurologists, psychiatrists, psychoanalysts, and physicians practising psychosomatic medicine have occupied themselves tirelessly with ' anomalies in the realm of mind '.

The scope of this volume is seen in the fact that a full third of it is concerned with theories of reincarnation, metempsychosis, or transmigration. The author reviews the various religious and philosophic doctrines on this subject and recounts several unusual episodes in which a person supposedly recalls his former life under the influence of hypnosis. Concerning one of these, the widely publicised case of ' Bridey Murphy ', Ducasse painstakingly endeavours to refute those who rejected it as an authentic instance of reincarnation.

In trying to establish his thesis that both discarnate survival and reincarnation are possible, the author argues at length to disprove the chief theories of mind that make it dependent on or a function of the activities of the body, nervous system, and brain. In this section of his book, I do not think he gives sufficient attention or importance to the numberless facts from the sciences of biology, physiology, psychology, and medicine that demonstrate an apparently indissoluble connection between the functioning of the physical organism, especially the cerebral cortex, and such mental processes as reasoning, imagining, and remembering.

The preservation of memory is of course indispensable to any future life that maintains the self-identity of the personality. But is is impossible to understand how a man's memory patterns—millions upon millions of them —imbedded in the interneuronic pathways of the cortex, could possibly continue to exist after the dissolution, decay, or destruction of the living brain where they had their original locus and being. And those stored-up psychophysiological memories that we call habits are likewise inseparably connected with the physical organism.

In the humanist or naturalist philosophy, stemming from Aristotle, that supports this general position, a human being has ideas, feelings, or emotions only with the occurrence and concurrence of ' certain complex physico-chemico-physiological events and structures '. From this viewpoint the abstraction ' mind ' always connotes an activity or function of the brain-body. It would be better to say mind*ing* or reason*ing* to show that thinking, without exception, is an activity of the physical organism as natural as breathing or digestion, and equally based on bodily processes. To over-simplify for the sake of emphasis, just as bad digestion can play havoc with effective thinking, so ' bad ' thinking in the form of extreme worry or mental depression, can adversely affect digestion and even lead

to ulcers. In both cases, within the complex and integrated whole that constitutes a man, the malfunctioning of one organ has deleterious effects on the functioning of another.

A fundamental difficulty in this entire problem is, as John Dewey has pointed out, that

> when we talk of the relations of mind *and* body, and endeavor to establish their unity in human conduct, we still speak of body *and* mind and thus unconsciously perpetuate the very division we are striving to deny. . . . In just the degree in which action, behavior, is made central, the traditional barriers between mind and body break down and dissolve. . . . What the facts testify to is not an influence exercised across and between two separate things, but to behavior so integrated that it is artificial to split it up into two things. (*Philosophy and Civilization*, New York, 1931, pp. 302-3.)

Disregarding Dewey's admonition, Ducasse treats mind and body as two different substances, agents or entities. He writes:

> Had not the word 'substance' so chequered a philosophical history, we could say that a mind is as truly a psychical substance as any material object is a physical substance. Let us, however, avoid the misunderstandings that this might lead to, and say that a mind, no less than a tree or sugar is a *substantive*—using this word . . . for the kind of entity to which the part of speech called a 'noun' corresponds (p. 56).

Thus, in his effort to pave the way for an after-life, the author, while not employing the traditional language, adopts a species of Cartesian dualism, although he does not say so specifically in this volume. He is much more definite in his earlier essay, 'In Defence of Dualism', published in the symposium, *Dimensions of Mind* (Ed. Sidney Hook, New York, 1960).

In attempting to disprove the monistic psychology that regards personality (including mind) as the activity of the body (as cuttingness is to the axe, in Aristotle's analogy), Ducasse spends considerable time on my book, *The Illusion of Immortality*. He contends that I have set up an 'ontological dualism' (p. 111) of my own when I state that the mind, in its manipulation of meanings, which are certainly not material objects, can and does influence the body. But mind, as I have repeatedly noted in this review, is always *man thinking*, the body-brain-cortex carrying on the function of apprehending, developing, imagining, recollecting, synthesising, putting into effect ideas. And these ideas or meanings do not originate or exist in some supernatural or other-than-natural realm. They are not apart from Nature, but *a part of* Nature; and operate through the language, signs, communication that complexly structured and thoroughly this-worldly organisms such as *homo sapiens* are able to establish.

For Ducasse to describe this well-known naturalistic position as an 'ontological dualism' is a radical misuse of terms. At the same time we can admit that there are completely naturalistic dualisms—if one prefers that word—within a man: for instance, between the distinctive functioning

of the brain and the rest of the body; or, within the brain itself, between the different functioning of the cortex and of the thalamus, cerebellum, and brain-stem.

At one point Ducasse becomes a bit polemical and accuses me of 'Turning aversions into disproofs' (p. 118). Yet I must state that far from having an aversion to a life beyond death, I, who have now reached sixty years of age, would like very much to awake after my demise to another existence, to see again the many dear friends and relatives I have lost, and to be able to believe in some dualistic psychology that makes all of this possible. In truth, I am one of those perhaps old-fashioned persons who desire immortality in the traditional sense of living on forever and ever. But trying to follow faithfully where facts and logic have led, I have been forced regretfully to give up my former faith in personal survival and to conclude that the probabilities against it are overwhelming.

My simple desire for immortality leads me to clash again with the author. For he evidently agrees with the dictum of Professor James B. Pratt that 'some sort of belief in at least the possibility of the object is a condition for any real desire for it'. I realise that many psychologists have the same opinion, but I am convinced they are wrong. Quite frequently I wish for things that I know cannot possibly be attained; and I think that most people have similar wishes.

I do not intend that my criticisms of Professor Ducasse's volume should seem to negate in any way the fact that he has mounted here one of the most powerful arguments I have encountered for the possibility of *post-mortem* survival. Although I have been unable to agree with some of his fundamental theses, I found his book fascinating to read and can recommend it to students of philosophy, religion, and eschatology as a most interesting and useful work.

CORLISS LAMONT

Roger Joseph Boscovich, S.J., F.R.S., 1711–1787. Studies of his Life and Work on the 250th Anniversary of his Birth. Edited by Lancelot Law Whyte. London, George Allen and Unwin, 1961. Pp. 230. 32s.

IT is not often that any of my prayers are answered. In 1957, in a footnote in a book, I called into the void for a reconstruction of Boscovich as a man, and now here comes this volume devoted to Boscovich, containing a Biographical Essay by Elizabeth Hill, Professor of Slavonic Studies at Cambridge. This Essay is a substantial piece of work, taking up over a third of the book, and presents the scientific career of Boscovich against the background of contemporary history. But the oracle has mocked me after all, for Professor Hill has not succeeded in giving this remarkable eighteenth-century figure the necessary blood-transfusion. The fault is not hers, for she writes (p. 18):

> This essay is based on printed sources mainly in Latin, Italian and Serbo-Croat, on countless articles scattered in Jugoslav periodicals and newspapers and on the few accessible unpublished sources; yet it is necessarily incomplete and psychologically Roger Boscovich remains largely unrevealed, for eighteenth-century material on him—his voluminous personal correspondence in particular—is still either inaccessible in known private hands or undiscovered in public archives.

Well perhaps after all my prayer was an unworthy one. Why try to reconstruct in human form one whose distinctive activities transcended human commonalty? Ask not what he was, but what he thought and did in science, and in what form those thoughts and actions inspire and influence the science of our day.

Those pregnant and pragmatic questions are answered in the eight short essays that make up the volume, in addition to a Foreword by Sir Harold Hartley, F.R.S., an Editorial Note by Mr L. L. Whyte, the Biographical Essay, a Bibliography, and an Index (rich in names but poor in subjects!). The titles and authors are as follows: 'Boscovich's atomism', by L. L. Whyte; 'Boscovich's *Theoria*', by Z. Marković; 'Boscovich and the British Chemists', by L. P. Williams; 'Boscovich and Priestley's theory of matter', by R. E. Schofield; 'The contribution of Boscovich to astronomy and geodesy', by Z. Kopal; 'Boscovich's mathematics', by J. F. Scott; 'Boscovich's optics and design of instruments', by C. A. Ronan; 'Boscovich and the combination of observations', by C. Eisenhart.

One might sum up by saying that Boscovich was two things—first, a competent and ingenious scientist, not really outstanding, and, secondly, a man with an idea. Had he been nothing but the former, no such volume as this would have been published. Had he been nothing but a man with an idea, the silence would have been still more complete, after a few derisory mutterings ('Crack-pot!') had died away. Einstein and Schrödinger each brought forward new ideas. They were accepted because, when applied quantitatively to physical problems, they gave correct quantitative results. But the idea of Boscovich had to flap its wings in the void as far as any quantitative check was concerned, because in his time, and for long after, experiment could not penetrate so far into the structure of matter. That the idea did not die was due, I believe, to the respect attached by his fellows to a competent and ingenious scientist. If he could speak well and understandably about what others understood, then perhaps his idea might prove to have something in it after all.

For competence and ingenuity three items may be cited. First, his derivation of an orbit from three finitely separated observations (p. 174)—one notes in passing his astronomical apology:

> Full of respect for the authority of the Holy Scriptures and the decrees of the Holy Roman Inquisition we shall consider the Earth as immovable . . . nevertheless, for reasons of mathematical simplicity we shall proceed as if it were moving.

Secondly, his method for the combination of observations, discussed by Dr Eisenhart with admirable conciseness and clarity. Thirdly, his proposal (and here we begin to accost the central idea) to measure the aberration of starlight by means of a telescope filled with water (p. 177), a project unfortunately abandoned on Boscovich's resignation in 1772 from the Brera Observatory, but carried out in 1871-72 by Airy at Greenwich—this experiment yielded one of those ' negative results ' easy to explain relativistically but not otherwise.

Now I come to the most difficult part of this review—the idea of Boscovich. To this idea he devoted his great book, the *Theoria*, republished in 1922 by the Open Court Publishing Company with the original Latin on one page and on the other an English translation by J. M. Child. It is the biggest scientific book I own—a bookcase-defying tome. How can I in a few words explain that which took the author so many?

Our search for Boscovich's idea is like the old lady's search for her glasses —she has pushed them up into her hair and that is the last place she will think of searching. Now I cannot answer for any member of the dismal tribe of philosophers ignorant of science into whose hands this review may come, but to any physicist or chemist I would say: To find the idea of Boscovich, look inside your own skull. It did not get there as part of the intellectual equipment of primitive man. It got there because Boscovich put it there—not directly nor quite in its original form, but filtered to its essence in time's great filter.

It was Boscovich who told us that, in contemplating the ultimate structure of matter, we might think of it as composed of moving points of no size at all, influencing each other by fields of force. How trivial this sounds to us today! Yet that is the great idea, a revolutionary idea upsetting the Newtonian idea of little hard impenetrable pellets—little, yes, but not *infinitely* small.

Was Boscovich right or wrong in his idea? Is matter really composed of point-particles? To my way of thinking (and I believe that here I am following the common trend) that is a silly question, the silliness residing in the word ' really '. We strive to understand nature by constructing mathematical models and we are pleased with those models which possess aesthetic simplicity and at the same time enable us to predict correctly (or fairly correctly) the results of carefully planned experiments. Modern physics relies largely on field theories in which something (better not ask what) is spread out in space and time. If that ' something ' is heavily concentrated around a point, it may become advisable to treat that concentration as a point of no size. If you do that, you are being Boscovichian.

The book is excellently produced and I warmly recommend it to readers, particularly those with curiosity as to the origin of the modes of thought in which they think without thinking about them.

J. L. SYNGE

Beweistheorie. (*Die Grundlehren der mathematischen Wissenschaften, Vol.* 103.) By K. Schütte.

Springer-Verlag, Berlin, 1960. Pp. x + 355. DM 48.00 (£4 7s. 6d.).

PHILOSOPHERS and scientists with any interest at all in symbolic logic cannot fail to obtain benefit from this book. The author's claim, in the foreword, that 'all the proofs . . . are either complete or so fully sketched that their completion is obvious' is borne out except in one or two places.

The formulations of logical calculi are unusual but this makes the book easier rather than more difficult to read. Rules of inference (*Schlußregeln*) are used in the spirit of Gentzen but a simplification is obtained by the use of positive and negative parts. These were first introduced in a more general way by Herbrand in his *Recherches sur la théorie de la démonstration* (Warsaw, 1930). The difference between the formulations is that in Herbrand's work every propositional variable occurs as either a positive or a negative part (or both), whereas in Schütte's a propositional variable need be neither a positive nor a negative part.

Syntactic variables are used almost exclusively and a propositional variable (or a well-formed formula) occurring as part of a [well-formed] formula is said to be a *positive* (*negative*) *part* if its taking the truth-value **t** (**f**) makes the whole formula take the truth-value **t**. Thus in the formula $\neg(p \wedge q) \vee r$, r occurs as a positive part and p and q occur as negative parts. Schütte writes $F[p_+]$ ($F[A_+, B_-]$) to denote that the propositional variable p (the formulae A and B) occur(s) as a positive part (as positive and negative parts, respectively) in the formula F. From the above definition it follows that if a propositional variable p occurs in a formula F (in different places) as a positive and as a negative part, then F always takes the truth-value **t** (whether p takes the value **t** or **f** in any assignment of truth-values). If every propositional variable in *each* occurrence is in fact a positive or a negative part (it cannot be both), then F is said to be *simple* (*einfach*). The simple formulae of the form $F[p_+, p_-]$ are taken as axioms for propositional calculus. (Not all tautologies are simple, e.g. $F[(p \wedge q)_+, (p \wedge q)_-]$.)

The rules of inference are:

$$\text{S1a. } F[A_+], F[B_+] \rightarrow F[(A \wedge B)_+],$$
$$\text{S1b. } F[A_-], F[B_-] \rightarrow F[(A \vee B)_-],$$

where $F[A]$ represents a formula in which A occurs and $F[B]$ denotes the result of substituting the formula B for the formula A at a specified occurrence of A in $F[A]$.

Using the converses of these rules, an immediate decision procedure can be given for the deducibility of formulae. If a formula is not an axiom then it is deducible if, and only if, it contains a positive part $(A \wedge B)$ or a negative part $(A \vee B)$ and is the conclusion of an S1 inference from the two

shorter formulae which (*i*) have A, B, respectively, replacing the part $(A \wedge B)$ or $(A \vee B)$ and (*ii*) are themselves deducible.

Chapter II is concerned with giving effective criteria for determining when rules of inference are derivable in various formal systems. Predicate calculus is treated in Chapter III and the completeness proof clearly shows where non-constructive elements come in. Number theory is discussed in Chapters V and VI, and Chapter V contains an elegant version of the consistency proof for arithmetic using the ordinal theory developed in Chapter IV. However, Professor Hao Wang has pointed out (in private conversation) that the formal system Ψ is not a formal system in the strict sense of the word, as the set of axioms is not decidable (cf. note on p. 146); but this does not affect the proof.

In order to avoid ω-incompleteness in wider theories, semi-formal systems, i.e. systems with an infinite induction rule, are introduced in Chapter VI. Chapter VII deals with transfinite induction and contains an erroneous assertion in theorem 23.12 which should read: If transfinite induction is deducible in Ψ for an ordinal α then α is less than the first ϵ-number. (For further technical details see the review by Kreisel to appear in the *Journal of Symbolic Logic*.)

Part III (Chapters VIII-XI) is concerned with various semi-formal systems culminating in a system **M** in which some constructive analysis can be formalised. The book closes with the remark that the system **M** is only to be taken as a *basis* for building a consistent analysis. Without exception the formal and semi-formal systems used in the book are proved consistent.

The treatment is self-contained even to the extent of scarcely mentioning the relation between the author's formulations of logical calculi and the more usual ones. But it is easy to see, via the completeness theorems, that the formulations of propositional and predicate calculi are entirely equivalent to the usual ones. Thus *Beweistheorie* is not suitable as an *introduction* to symbolic logic at present. Nevertheless it is to be hoped that the author's presentation of logical calculi will be generally adopted by all teachers of logic in the near future.

Another point of obscurity concerns the ordering relation $\prec$. This relation has the advantage of ensuring a *unique* representation for each ordinal that is represented at all (unlike the Kleene system using $<_0$) but suffers from opaqueness. For clarification the reader is referred to Schütte's 1954 paper and the paper which contains the origin of the $\prec$-relation, namely, O. Veblen's *Continuous increasing functions of finite and transfinite ordinals* (*Trans. A.M.S.* 1908, **9**, 280–92), but which is not cited in the bibliography.

It is to be hoped that the language and price barriers will not prevent (parts of) this book being read by *all* logicians. It would be difficult to imagine any mathematical logic more clearly presented.

There are few misprints. The reviewer has noticed the following:

p. 50, l. 17: For 'zusässig' read 'zulässig'.
p. 84, l. 20: For '$((A \vee B) \vee C)$' read '$((A \wedge B) \vee C)$'.
p. 90, l. 5: Insert '[' after the second 'F'. In 4b., for '$\forall x A(a)$': read '$\forall x A(x)$'.
p. 149, l. 15: Insert '(' before '$A \vee B$'.
p. 153, l. 19: For 'Satzes 8.4' read 'Satzes 8.2'.
p. 176, last line: For 'B' read '$B_$'.
p. 193, 5th line up: For '3)' read '2)'.
p. 286, l. 8: For ')]' read '))]'.
p. 287, l. 19: For 'zuässigen' read 'zulässigen'.
p. 309, 8th line up: For '$\gamma_1 + \gamma_2$' read '$\gamma_1 + \gamma_2$'.
p. 321, 2nd line up: For '$(\gamma \in m \ldots$' read '$((\gamma \in m) \ldots$'.
p. 322, l. 20: For '$(b \wedge m)$' read '$(b \in m)$'.

J. N. CROSSLEY

Man's Picture of his World. By R. E. Money-Kyrle. Duckworth, London, 1961. Pp. 190. 21s.

THIS serious, conscientiously thought-out essay in psycho-analysis for philosophers describes the development of instinct in the child in terms of a developing world-model: every little boy or gal is, successively, a subjective monist, a naif realist and a potential Hume. Finally the world-model becomes our instrument for understanding others, to whom we attribute minds like our own; the psycho-analyst understands his patients through himself—and with particular penetration because the self he projects into their shoes has been analysed already. Accepting, on behalf of psycho-analysis, the Popper-prompted challenge that if a science is genuine it must be possible to formulate the evidence which would refute it, the book starts by considering how an analytic interpretation might prove false: an error by the analyst will reflect a foggy patch in his own analysis and a distortion in the world-model which is his analytic instrument.

Whether the analyst's assumptions and self-knowledge could be wholly contradicted, rather than corrected in part, is a crucial bridge not quite crossed. But the author goes a long and valuable way when he makes out an advance probability in favour of psycho-analysis from the standpoint of biology, a science which has not yet necessarily taken the step of imputing minds to others: what psycho-analysis finds through its questionable and subjective instrument tallies with what one would expect from the objective consideration of evolution. Freud's Eros and Thanatos instincts are reinstated (though processed, like all his concepts in this book, through Melanie Klein's rather genteel and under-realised terms of good and bad breasts, which scarcely seem adequate to Freud's 'battle of the Titans'). The quest to understand how a suicidal death instinct can have been introduced in the service of life is possibly misplaced: the rewarding place to

look may be not the formal contrast between life and death but the conflict between species- and individual-preservation. A self-preservative species must retain evolutionary adaptability; and since this functions in the replacement of one generation by the next, the species must frame its individuals' instinctual endowment in such a way that they will submit to being written off. The author makes a true and vitally important point when he traces the unique crisis of Eros and Thanatos in man to the mammalian peculiarity that the baby really does attack and eat the mother; and he convincingly suggests that the crisis, potential in all mammals, is sharpest in man because his social organisation obliged him to go back on a prohibition against preying on his own kind.

Having shown the analyst's errors as the result of distortions in his own world-model, the author suggests that not only are many of our factual mistakes produced in the same way but many disputes which seem to be about values are really about facts: the disputants are not disagreeing over their estimates of a given object but are, through faulty instruments, seeing different objects. Applying this to ethics, whose judgments are, at least by genesis, social, the author is illuminating. It leads him to naiveté only in aesthetics, which he tries to 'rescue' from the relativist argument by his thesis that a group of 'mature', psycho-analysed critics would come near to agreeing on a list of the most satisfying works of art. No doubt they would: the doubt is whether they would be right. The author believes they would recognise and opt for the most mature works, but this would not deliver us from relativism at all unless it were shown by some more than relative standard that the most mature works are necessarily the best. Most people concerned enough to have an artistic taste of any kind recognise that in art 'maturity' is a hardly less dangerous criterion than 'normality'. A mature Baudelaire would be valueless. A (presumably) mature Rimbaud stopped writing. Indeed, it may be that maturity actually makes for not merely bad art (a relative judgment) but no art (a factual one): whether the results are good or bad in either case, most children but few adults paint pictures. The author acknowledges the danger of making the mature, 'integrated' person his artistic hero when he admits 'Many artists have been ill people', yet he allows himself the naif reply 'But those, like Bach, who were not ill, have probably achieved the most'. Whether we read *ill* in the mental or the bodily sense, and *most* in the quantitative or the qualitative, or both in both cases, Mozart, Watteau, Keats, and Proust rebuke him from their deathbeds.

This, however, is to carp against a small section of a modest, persuasive and interesting book, written without inspiration but also without jargon or (apart from the footnote which uses *strata* as a singular) offence, and not only clear but fair.

BRIGID BROPHY

Biology, Psychology and Belief. By W. H. Thorpe.
Cambridge University Press, 1960. Pp. 60. 4s. 6d.

EPISTEMOLOGY, it may plausibly be argued, is a branch of ethology, and so, for that matter, is ethics. From the nature of his profession, therefore, one would expect Dr Thorpe's Eddington lecture to bear on a number of philosophical problems. In fact, as his title indicates, he has covered a very wide range indeed, linking art and religion as well as philosophy with the implications of modern biological research. Any one of the points he makes would merit much more extensive and serious study than is possible in this brief review; I can do little more than list some of them here.

First, Dr Thorpe reaffirms Sherrington's concept of the brain as the ' organ of liaison ' between mind and body. Recent advances in the application of physical and chemical concepts to biological problems by no means deflect him from this dualistic conception. Thus while recognising that ' the living machine is a marvel of chemical engineering ', he believes that the new laws of physics and chemistry which would be found to govern such a living machine, should it be produced in the biochemist's laboratory, would vindicate the doctrine of emergence. Brain would still appear as the organ of mind, rather than as mind itself. Paradoxically, the thesis (suggested by recent experiments on anabiosis) that life depends on microstructure rather than on function seems to Dr Thorpe to strengthen rather than weaken his dualistic view. He also mentions with approval, however, the neurological dualism of Eccles, which seems to point to ' activity ' rather than structure as the carrier of the ' mental ' aspect of living processes. This is rather confusing, and one would like to know how Dr Thorpe envisages the relation of structure and function here.

The mind-brain problem leads inevitably to the question of mind in men and other animals. Of particular interest are Dr Thorpe's reference to play as a criterion of mental evolution and his discussion of the beginnings of the ' artistic sense ' in the animal world. One is also grateful to Dr Thorpe for his candid statement that ' the concept of mind is a valuable tool in the designing of experiments on animal behaviour '. On the difference between the minds of animals and men, Dr Thorpe somewhat misinterprets Polanyi's *Personal Knowledge*, for the distinction as Polanyi makes it agrees entirely (if I understand them both) with the distinction Thorpe is making in this passage: the difference between human and animal minds, Thorpe writes, is ' a difference in the degree to which the symbol becomes freed from the object so as to become manipulable as part of an intellectual world '. It is just this difference (analysed in Chapter Five of *Personal Knowledge*) which makes possible the emergence of human intellectual passions, with their universal intent, over and above animal appetites: the distinction which Thorpe seems to call in question.

On the broadest aspect of his subject, ' the main " general message " . . . of recent developments in biological science ', Dr Thorpe pleads for the unity of science, art, and religion and emphasises the central rôle of *Gestaltung* in all our efforts to evaluate and understand the world around us. Although the reader may wish for some elucidation of the ' real *a priori* knowledge ' on which, in Dr Thorpe's view, all judgment rests, this reader at least is grateful for his authoritative dissent from the cult of the two cultures.

The weakest part of the lecture, I feel, is in the discussion of evolution in general (as distinct from the evolution of mind). Dr Thorpe accepts too readily the shallow arguments about the ' creativeness ' of natural selection and the comforting ' indeterminacy ' of the physical world.

MARJORIE GRENE

Essay on Atomism: From Democritus to 1961. By Lancelot Law Whyte. Nelson and Sons Ltd., London and Edinburgh, 1961. Pp. 108. 16s.

IT is a remarkable fact, which Mr Whyte does not fail to point out, that so far in the present century no one has written a definitive history of atomism in any language. Indeed, the last truly comprehensive book on the subject appears to have been K. Lasswitz's classic *Geschichte der Atomistik*, published in 1890—nearly three quarters of a century ago! In recent years there have been valuable memoirs by Professor J. R. Partington (' The Origins of Atomic Theory ', *Annals of Science*, 1939, 4), Professor S. I. Vavilov (' Newton and Atomic Theory ', in the Royal Society Newton Tercentenary Celebration volume, 1947), and Professor Marie Boas (' The Establishment of the Mechanical Philosophy ', *Osiris*, 1952, 10), but the only book covering the entire history of the subject has been van Melsen's philosophically oriented *From Atomos to Atom: the History of the Concept Atom*, published in 1952.

In view of this, Mr Whyte has performed a useful service in presenting in crisp, compact, and easily readable form an outline of the main stages in the evolution of atomic ideas. Although not everyone will unreservedly accept his opening statement that the conception of atomism has been ' the spearhead of the advance of science ' since it may be argued that dynamical concepts have played a no less crucial rôle, Mr Whyte's enthusiasm has led him to explore the literature to good effect and his summary account should be a valuable work of reference for both scientists and students of the history and philosophy of science.

The book is divided into six chapters. In the first, the appropriate moral is drawn that the origin of atomism justifies the power of speculative philosophy. An interesting parallel is drawn between the modern transformation of ideas in physics and psychology, modern elementary particles

differing from the permanent *res extensa* of Descartes no less than the modern concept of mind differs from the Cartesian *res cogitans*, both indicating a revolution in thought away from permanent 'substances' to changing 'structures of relations'. Mr Whyte is, however, alarmed at the increasing abstractness of modern particle physics and would welcome 'a return to concreteness of basic ideas, to simpler fundamentals easily understood, to principles that will bring exact science closer to the human person'.

The other main point which the author makes in this chapter is that students of physics should know something of the history of atomism. He advances a two-fold justification for this plea. A balanced education depends on seeing the present day in historical perspective. (When Rutherford first took over the Cavendish Laboratory, he was shocked to discover that the younger generation thought that science began in their day.) Another reason for studying the history of atomism, in Whyte's view, is that a clue to the future can sometimes be found in the past by uncovering and eliminating unnecessary features which have been unwittingly incorporated in physical theory.

In Chapter 2, the idea of atomism is examined and its various forms contrasted: Democritan—Newtonian 'billiard balls', Boscovichian point-centres, and de Broglie-Schrödinger wave-particles, etc. The idea of particles as field singularities is also admitted, but its limitations are stressed. Chapter 3 is basically a compendium of information on the history and the geography of atomic ideas, attention being focused on the fact that since the seventeenth century atomic theories have largely depended on the combination of Pythagorean and Democritan concepts.

In Chapter 4 we come to the heart of the book. This is a detailed chronological table, interrupted from time to time for commentary and exegesis. In Chapter 5, 'Analysis', the author draws his conclusions from the story outlines in the previous chapters, emphasising the subtle transformations which the atomic concept has undergone. Two contrary tendencies are noted in modern times: although an increasing number of properties and parameters have been assigned to ultimate particles (mass, charge, spin, life-time, parity, strangeness, etc.), the ultimate aim has not been forgotten of reducing the number of these independent theoretically arbitrary parameters. In the final chapter, Whyte speculates on the future and wonders whether the field or its sources will be regarded as theoretically primary. There is also an annotated selected bibliography covering historical surveys in English, French, and German.

The book is clearly printed and should prove a welcome addition to the shelves of all those interested in the subject.

G. J. WHITROW

ABSTRACTS

Philosophy of Science, 1960, **27**, No. 1

J. Kotarbinska, ' On Ostensive Definitions '

The first part deals with the problem of the external form of ostensive definition. It is concluded that the definition statement is not complete. The proper form of this statement is not a sentence, but a sentential function of the type: '$\pi x|N(x) = x$ is in the respect R and in the degree D such as A, B . . . and not such as K, L . . .]' where ' N ' stands for the term being defined.

In the second part the conditions are analysed on which depends the possibility of solving that problem, i.e. the conditions on which depend the informational value and the efficacy of ostensive definition.

S. Nowak, ' Some Problems of Causal Interpretation of Statistical Relationships '

An attempt is made to analyse the statistical relationships between variables as the functions of causal relations existing between them. The first part of the paper presents basic concepts which may serve for the description of different patterns of causal relations. The second part of the paper is an application of proposed method to the understanding and critical consideration of some standard techniques of statistical analysis, especially those mostly used in social sciences.

H. W. Burns, ' Pragmatism and the Science of Behavior '

Many pragmatic philosophers insist that causality in human behaviour is to be explained by psychological field theorism rather than by modern behaviourism. This paper attempts to demonstrate (1) that pragmatists often support one aspect of an untenable disjunction in psychological theory, (2) that the asserted disjunction is but a methodological distinction, and (3) that the causal order in human behaviour is most likely to be profitably described, predicted, or explained by the methods of modern behaviourism.

A. Grünbaum, ' The Duhemian Argument '

This paper offers a refutation of P. Duhem's thesis that the *falsifiability* of an isolated empirical hypothesis H as an *explanans* is *unavoidably inconclusive*.

N. Rescher, ' The Problem of a Logical Theory of Belief Statements '

It is shown that the logical theory of belief statements must be prepared to take into account relationships among statements which are subtler and more delicate than is requisite in other contexts. It is necessary here to draw distinctions (of a modal and semantical character) which the standard assertory logic can ignore with impunity.

Philosophy of Science, 1960, **27**, No. 2

L. Durand III, ' On the Theory of Measurement in Quantum Mechanical Systems '

This paper is concerned with the description of the process of measurement within the context of a quantum theory of the physical world. Quantum mechanics

permits a quasi-classical description (classical in the limited sense implied by the correspondence principle of Bohr) of those macroscopic phenomena in terms of which the observer forms his perceptions. Thus, the process of measurement in quantum mechanics can be understood on the quasi-classical level by transcribing from the strictly classical observables of Newtonian physics to their quasi-classical counterparts the known rules for the measurement of the former. The remaining physical problem is the delineation of the circumstances in which the correlation of a peculiarly quantum mechanical observable A with a classically measurable observable B can result in a significant measurement of A. This is undertaken within the context of quantum theory. The resulting clarification of the process of measurement has important implications relative to the philosophic interpretation of quantum mechanics.

S. and A. Naess, ' Psychological Research and Humean Problems '

The question is raised whether philosophers, studying Humean problems, might profit from the empirical findings of contemporary psychology. A text from Hume's *Treatise of Human Nature* is analysed in an attempt to find out (1) whether his problems are open to empirical testing and (2) whether contemporary empirical psychology has contributed to the testing of Hume's hypotheses.

F. M. Fisher, ' On the Analysis of History and the Interdependence of the Social Sciences '

The views of some historians and philosophers of history as to the possibility of fruitful historical generalisation seem at odds with the underlying methodology of the other social sciences. A formal model of the world historical process is here presented within which this apparent contradiction is seen to be resolvable in terms of modern theories of probability and stochastic processes.

W. W. Rozeboom, ' Do Stimuli elicit Behavior?—A Study in the Logical Foundations of Behavioristics '

Modern behaviourists speak of stimuli as though they elicit responses from organisms. Logically this is absurd, for analysis of the grammatical rôles of stimulus and response concepts shows that stimuli and responses differ in logical type from causes and effects.

D. R. Luce, ' The Action of Mind on Body '

Terminology and symbolism are introduced, which facilitate the precise statements of propositions concerning the action of mind on body. It is noted that radical interactionism, or ' exceptionalism ' as it is here called, is a contingent, empirically-decidable issue which is quite independent of metaphysical views regarding ' mind ' and ' matter '. For that very reason it should not be the object of special philosophic concern.

Philosophy of Science, 1960, **27**, No. 3

H. Dingle, ' Relativity and Electromagnetism: An Epistemological Appraisal '

It is shown that the experiments on which Margenau and Mould rely to establish the special theory of relativity actually confirm the mutual consistency of the Maxwell-

Lorentz electromagnetic theory and the special relativity theory, but throw no light on the validity of the two theories taken jointly. Also that a modification of the rules of correspondence between the mathematical structure of the theories and immediate experience would bring the theories into agreement with an alternative relativity theory based on the Galilean instead of the Lorentz transformation. An experiment is suggested by which the need for such modification can be tested. A proof is then given that the rules of correspondence between the concepts of the special relativity theory (and therefore of current electromagnetic theory) and experience are not self-consistent, so that some modification of current ideas is essential. It is suggested that a generalisation of Maxwell's theory, in terms of Faraday's 'ray vibrations' instead of Lorentz's static ether, might provide a satisfactory basis for a relativistic electromagnetic theory.

E. D. Klenke, 'Universals and Particulars in a Phenomenalist Ontology'

A phenomenalist philosophy which employs the Principle of Acquaintance (PA) plus the Principle that what exists are the referents of certain meaningful terms, defined by PA, cannot include either universals or particulars in its ontology, but is limited to *instances* of universals as constituting the range of ontological existents.

M. Bunge, 'The Place of Induction in Science'

The place of induction in the framing and test of scientific hypotheses is investigated. Two kinds of induction are distinguished: the inference of generals from particulars (first degree induction), and the generalisation of generalisations (second degree induction). Induction is claimed to play a rôle in the framing of modest empirical generalisations and in the extension of every sort of generalisation—not however in the invention of high-level hypotheses containing theoretical predicates. Refutation is not viewed as a substitute for confirmation but as its complement.

L. E. Palmieri, 'Pragmatism and the Ideal Language'

The author opposes the view that the analytic-synthetic distinction rests on mere convention, because (1) the natural language firmament is itself an amorphous construct, (2) the natural language might be the language of experience but not of empiricism, (3) the ideal language is tied to experience by primitives for what perceptually appears, and (4) if one claims the primitives must be tested in scientific enquiry, a case should be made for this *philosophical* position.

H. N. Castaneda, 'On a Proposed Revolution in Logic'

In his *The Uses of Argument* (Cambridge, 1958), S. Toulmin presents serious charges against ordinary logical theory. The paper argues that none of the charges is proven, that most of them cannot be proven, that Toulmin's new logic is at best only vaguely hinted at and that his suggestions are positively obscure or mistaken.

H. A. Finch, 'Confirming Power of Observations Metricized for Decisions among Hypotheses'

Experimental observations are often taken in order to assist in making a choice between relevant hypotheses $\sim$H and H. The *power of observations* in this decision is here metrically defined by information-theoretic concepts and Bayes's theorem.

ABSTRACTS

Philosophy of Science, 1960, 27, No. 4

R. W. Sellars, ' Panpsychism or Evolutionary Materialism '

Extended reference is made to C. S. Peirce's view of perception as realistic in intention and yet not quite clear as to its mechanism and how it attains objective import.

A. C. Benjamin, ' Is the Philosophy of Science Scientific? '

The philosophy of science should, at times, become self-critical. When a study is concerned primarily with methods of other disciplines it tends to under-emphasise the rôle played by the methods which it itself uses. What *is* the method of the philosophy of science, and is it justifiable? Can there be a satisfactory study of science and can this be scientific?

W. W. Rozeboom, ' Studies in the Empiricist Theory of Scientific Meaning. Part I —Empirical Realism and Classical Semantics: A Parting of the Ways. Part II— On the Equivalence of Scientific Theories '

Part I is concerned with the tenet of modern Empirical Realism that while the theoretical concepts employed in science obtain their *meanings* entirely from the connections their usage establishes with the data language, the *referents* of such terms may be ' unobservables '.

Part II examines a possible axiomatic basis for the empiricist theory of scientific meaningfulness to demonstrate that even if theoretical terms are able to *designate* entities inaccessible to the observation language, as held by Empirical Realism, so long as the *meanings* of theoretical terms derive from their connections with the observation language, the meaning content of a theory is exhausted by its observational consequences.

R. G. Turnbull, ' Imperatives, Logic, and Moral Obligation '

It is claimed that ' Do x! ' means ' Then you will do x'. Answering a ' Why? ' question concerning the former may take either of two forms, viz., ' Because. . . . ' or ' If you wish to. . . . ' The second answer completes the truncated hypothetical. ' Ought ' sentences are treated as a species of imperative involving universality, in the ' if ' clause. Moral ' ought ' sentences involve a *double* universality, viz. the one mentioned above *and* universality connecting the action with social harmony.

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THE PARADOX OF CONFIRMATION*

J. L. MACKIE

THE paradox of confirmation, also known as the paradox of the ravens, has been much discussed.[1] But the sound points that have been made, I believe, by all the contributors are in danger of being lost in the controversy, and particularly in the mutual misunderstandings and charges of misunderstanding. I shall, therefore, try to place in order some of the main points that should emerge from the discussion and also to develop some further considerations.

The paradox is constituted by the three propositions :

(1) Observations of black ravens confirm ' All ravens are black '.

(2) Observations of black shoes, white swans, etc., are neutral to (i.e. do not confirm) ' All ravens are black '.

(3) If observations confirm one formulation of a hypothesis they confirm any logically equivalent formulation.

These three propositions all seem plausible, but they are incompatible; for an observation of a white swan is an observation of a non-black non-raven, this is related to 'All non-black are non-ravens ' in the same way that the observation of a black raven is to ' All ravens are black ', and these two universal propositions are equivalent. Since no-one will reject (3) or deny the equivalence of the two universals, the paradox can be solved only by rejecting or qualifying (1) or (2) or both. Professor Hempel rejects (2) ; Mr Watkins qualifies both (1)

* Received 13.x.61

[1] See, for example : Janina Hosiasson-Lindenbaum, *Journal of Symbolic Logic*, 1940, **5**, 133-148 ; Carl G. Hempel, *Mind*, 1945, **54**, 1-26 and 97-121, also *Philosophy*, 1958, **33**, 342-348 ; J. W. N. Watkins, *Philosophy*, 1957, **32**, 112-131 and 1958, **33**, 349-355, also this *Journal*, 1960, **10**, 318-320, also *Australasian Journal of Philosophy*, 1959, **37**, 240-241 and 1960, **38**, 54-58 ; D. C. Stove, *Australasian Journal of Philosophy*, 1959, **37**, 149-151 and 1960, **38**, 51-54 ; H. G. Alexander, this *Journal*, 1958, **9**, 227-233 and 1959, **10**, 229-234 ; J. Agassi, this *Journal*, 1959, **9**, 311-317 ; I. J. Good, this *Journal*, 1960, **11**, 145-149 and 1961, **12**, 63-64.

and (2), and uses the difficulties he sees in Hempel's solution as a support for Professor Popper's theory of confirmation.

For simplicity I shall exclude, for a time, the complications involved in the use of such terms as ' white ', ' shoes ', ' swans ', etc., and take it that (2) asserts that observations of black non-ravens and non-black non-ravens are neutral to the hypothesis (*h*) that all ravens are black.

1

If we consider this problem in the setting in which Hempel proposed it, namely, without reference to any additional knowledge, then we must say that if the observation of a black raven confirms *h* then an observation of a non-black non-raven equally confirms *h*. But in a setting as bare as this there could be no better confirmation of *h* than the observation of black ravens. So if we are to define confirmation in such a way that it can occur in relation to a context of this sort, we must accept (1) and therefore reject (2). And we can do so, because in this context the rejection of (2) is not counter-intuitive ; we simply have no intuitions in favour of (2) that are relevant to a setting as bare as this. Thus Hempel's solution of the paradox is adequate for its context ; the paradox is not a difficulty for his definition of confirmation in its original setting, and he is justified in using the paradox to reject Nicod's definition according to which a black raven would confirm *h* but a non-black non-raven would not.

But whether we can plausibly speak of confirmation in a setting as bare as this, or, what comes to the same thing, whether any definition of confirmation that gives positive results in this setting can be carried over to ordinary contexts or will even throw light on confirmation in ordinary contexts, is a further question.

2

If we take account of the additional information that there are far more non-black things than ravens, then we can solve the paradox in another way. That is, we can still deny (2), but argue that the observation of non-black non-ravens is not nearly so good a confirmation of *h* as the observation of black ravens, and that, being intuitively aware of the relative weakness of the former confirmation, we mistake it for a complete failure to confirm, and so we have, in this context, a mistaken intuition in favour of (2).[1]

[1] Hosiasson-Lindenbaum, Alexander, and Good all develop variants of this way of solving the paradox, though they also make other points.

This explanation applies generally, for although it is only if there are far more non-ψ's than ϕ's that the observation of something that is both ϕ and ψ is really a much better confirmation of (x) $[\phi x \supset \psi x]$ than the observation of something that is both non-ϕ and non-ψ, we are so used to giving affirmative terms to minority classes (if ϕ is an affirmative term, there are nearly always far more non-ϕ's than ϕ's) that we can understand how a mistaken intuition in favour of the generalisation of (2) could arise.

It is important to see, however, that to develop this solution we have to appeal to what I shall call the *Inverse Principle*, that a hypothesis h is confirmed by an observation-report b in relation to background knowledge k if and only if the observation-report is made more probable by the adding of the hypothesis to the background knowledge, that is, if the probability of b in relation to the conjunction of k and h is greater than the probability of b in relation to k alone—in symbols, if $p(b,\ k\ .\ h) > p(b, k)$—and that b confirms h the better the more the adding of h to k raises the probability of b.

For example, in one of Alexander's accounts of this solution, we assume this principle and assign probabilities in accordance with the relative size of the classes ' ravens ', ' non-ravens ', etc. Thus if we take it as known that the ratio of ravens to non-ravens is x to $1 - x$, and that of black things to non-black things y to $1 - y$, where $x < y < \frac{1}{2}$, then the probabilities of each of the four sorts of observation-report b_1 (' This is a black raven '), b_2 (' This is a non-black raven '), b_3 (' This is a black non-raven '), and b_4 (' This is a non-black non-raven '), in relation (i) to our background knowledge k alone, and (ii) to the conjunction of k with h, are as set out below:

	b_1	b_2	b_3	b_4
(i)	xy	$x(1-y)$	$y(1-x)$	$(1-x)(1-y)$
(ii)	x	0	$y-x$	$1-y$

Then clearly reports of the form b_1 have their probability raised by the adding of h far more than those of the form b_4, and so b_1 reports confirm h far better than b_4 ones do, while a b_3 report is actually made less probable by the adding of h to the k assumed here, and thus, in relation to *this* background knowledge, disconfirms h, though only slightly.[1]

[1] I have followed one of Alexander's accounts, but his other account, and the more elaborate accounts of Hosiasson-Lindenbaum and Good, all rest upon theories which involve the Inverse Principle and the assigning of probabilities in accordance with the distribution of characteristics.

In this setting also, then, we can solve the paradox by denying (2) and explaining away our intuitions in favour of it. It follows that the paradox cannot be used against an account of confirmation of this sort—an account which does not equate confirming with satisfying a hypothesis, but which bases it on the Inverse Principle and an assignment of probabilities in accordance with a taken-as-known distribution of characteristics. But it is still a further question whether confirmation defined thus, for a context of still very severely limited knowledge, has much to do with confirmation in ordinary contexts. But this qualification works in two directions. If we accept this account in its proper context, we cannot go on and apply it unhesitatingly in other contexts. But equally we should not use difficulties in its application to other contexts to throw doubt on the account itself.

Thus this account of confirmation does not imply that observations of black ravens, however numerous, in ordinary contexts will provide a worthwhile confirmation of *h*, still less that such observations of non-black non-ravens will do so, or even that they will confirm *h* at all, especially if they are actually observations of white swans, etc., which involve further terms which may possibly be related to *h* in other ways.[1] But for this very reason our commonsense doubts whether observations of non-black non-ravens in ordinary contexts could confirm *h* do not reflect adversely upon this account of confirmation or upon this solution of the paradox in its own context.

Thus to deny (2) in this still restricted context does not commit us to any absurd practical policies of investigation, such as trying to confirm *h* by cheaply scoring up enormous numbers of non-black non-ravens, and the absurdity of this policy in no way tells against the denial of (2) in this context.[2] Nor does it commit us to a policy of trying to support currently-accepted hypotheses as opposed to a policy of trying to overthrow them. And even if we wanted to support a certain hypothesis, the fact that a certain kind of observation would, in relation to severely limited background knowledge, confirm that hypothesis, does not imply that a sensible way of supporting it would be to *try* to make observations of that kind ; for *trying* to make a certain kind of observation alters our background knowledge, and (as I shall show in section 4 below) may alter it in such a way that observations

[1] Cf. Good's distinction between ' stoogian ' and ' non-stoogian ' observations.

[2] Watkins and Agassi repeatedly use this improper extension to discredit the ' instantial theory '. See, e.g. Watkins in *Philosophy*, 1958, **32**, 118, and Agassi in this *Journal*, 1959, **9**, 315.

of that kind now confirm the hypothesis much less, or perhaps not at all.

3

It would seem, then, that Hempel-type solutions of the paradox, denying only proposition (2), can be defended, at any rate within some limited contexts. But Mr Watkins has a strong argument in favour of the view that both (1) and (2) should be qualified,[1] which I would restate as follows.

Suppose that there is an observation-procedure which could turn out in just one of only two possible ways. For example, we inspect an object, already known to be black, to see whether it is a raven or not ; the outcome must be either that it is a raven or that it is not. Both the alternative outcomes cannot confirm the same hypothesis : if the observation's having turned out one way confirms the hypothesis, its having turned out the other way would not have confirmed it. This, which I shall call the *Alternative Outcome Principle*, is obvious enough in its own right ; it is also provable as a consequence of the Inverse Principle. For in relation to a k which includes the fact that a procedure with just these alternative outcomes has been followed, the denial of one outcome is equivalent to the assertion of the other ; so if b confirms h, then adding h to k makes b more likely, and therefore makes not-b less likely, and so the alternative outcome, not-b, would not confirm h but disconfirm it.

From this it follows that if we inspect an object already known to be black, its turning out to be a raven and its turning out to be a non-raven cannot both confirm h, though both are compatible with or satisfy the hypothesis ; to confirm a hypothesis, then, an observation must do something more than satisfy it.

This seems to show that b_1 and b_3 observations, since they could be such alternative outcomes, cannot both confirm h, and it suggests the following further conclusions. If we inspect an object already known

[1] *Philosophy*, 1957, **32**, 119–120, and this *Journal*, 1960, **10**, 319. Watkins has not always made it clear that his conclusion is that both (1) and (2) should be qualified. On p. 116 of the article in *Philosophy* he describes his way out as being to 'deny the instantiation theory of confirmation implicit in proposition (1)', and one might be forgiven for supposing that he was denying or modifying (1) only. But subsequent discussion has brought it out that his account qualifies (2) also, saying that h *may* be confirmed by a b_4 observation if it is made in a *test* of h.

to be a raven and it turns out to be black, this confirms *h*, for the procedure might have turned out the other way and falsified *h* ; but if we inspect an object already known to be black and it turns out to be a raven, this does not confirm *h*. Similarly, if we inspect an object already known to be non-black and it turns out to be a non-raven, this confirms *h*, for the alternative outcome would have falsified *h*, but if we inspect an object already known to be a non-raven and it turns out to be non-black, this does not confirm *h*. And on this view there is no order of inspection by which the observation of a black non-raven could confirm *h*. Thus both (1) and (2) should be qualified.

But these further conclusions are paradoxical. If we adopt in this sense the view that to confirm a hypothesis a procedure must first endanger it, we make confirmation depend not on what we observe but on the order in which we notice characteristics. A black raven observed in one way ('Look, a raven—and it's black') confirms *h*, but observed in another way ('Look, something black—why, it's a raven') it does not. And this must be wrong. We might concede that the way in which an observation is made could affect its confirmatory power, but only if the 'way' involved more significant differences than this. The mere order in which characteristics are spotted must be an irrelevant difference. If confirmation is to be an objective relation, and not a psychological one, the total observation of a black raven must have the same confirmatory value in whichever order its components are put together. In other words, the 'further conclusions' suggested by the Alternative Outcome Principle fall foul of a *Principle of Relevance*.

Part of this difficulty is removed if we distinguish carefully the various 'procedures' and 'outcomes'. The procedure of inspecting something already known to be a raven, with the alternative outcomes that it is, or is not, black, is only the second stage of the observation of a black raven ; before it there was another procedure, in which a previously uncharacterised object turned out to be a raven, but might have turned out to be a non-raven. The observation of a black raven can be regarded as being made up of two stages, then, in either of the following ways :

1. (1.1) This (previously uncharacterised object) is a raven, and (1.2) This raven is black.
2. (2.1) This (previously uncharacterised object) is black, and (2.2) This black thing is a raven.

Similarly, the observations of a black non-raven and of a non-black

non-raven can be divided into stages as follows :

3. (3.1) This is black, and (3.2) This black thing is a non-raven.
4. (4.1) This is a non-raven, and (4.2) This non-raven is black.
5. (5.1) This is a non-raven, and (5.2) This non-raven is non-black.
6. (6.1) This is non-black, and (6.2) This non-black thing is a non-raven.

This analysis makes a solution at least formally possible, for our 'further conclusions' from the Alternative Outcome Principle have stressed differences in the confirming effect of the various second stages, such as 1.2 and 2.2, but the Principle of Relevance requires only that 1 as a whole, the combination of 1.1 and 1.2, should have the same confirming effect as 2 as a whole, the combination of 2.1 and 2.2. Clearly, 2 as a whole could be as good a confirmation as 1 as a whole, even if 1.2 was superior to 2.2, provided that 2.1 was correspondingly superior to 1.1.

We cannot, however, save the extreme view expressed in our 'further conclusions' in this manner. For if 2.2 did not confirm *h* at all, we should have to say that 2.1 alone confirms *h* as well as 1 as a whole does, which is absurd, especially as the reason given for saying that 2.2 does not confirm *h* at all, that the alternative outcome would not falsify *h*, would equally tell against 2.1.

This extreme view, then, must be rejected, and it can be rejected without regret since it is not required by the Alternative Outcome Principle. That principle tells us only that one outcome can confirm only if the other outcome would disconfirm, not that the other outcome would have to falsify.[1] But a more moderate view can be worked out as follows.

1.2 is in general a better confirmation of *h* than 2.2, but 2.1 is in general correspondingly better than 1.1, so that 1 as a whole and 2 as a whole are, as the Principle of Relevance requires, equally good.[2]

[1] As Watkins suggests in this *Journal*, 1960, 10, 319 : '. . . *could not possibly* lead to its falsification and hence that the results of such an investigation, whatever they might be, could not confirm it according to a testability theory of confirmation.'

[2] An artificial example may be sufficient to illustrate this. Suppose that our background knowledge tells us that 10 per cent of objects are ravens and that 10 per cent of non-ravens are black. Then the probable distributions in relation to (i) this knowledge alone (ii) this knowledge plus *h*, are as follows :

	black ravens	non-black ravens	black non-ravens	non-black non-ravens
(i)	1/100	9/100	9/100	81/100
(ii)	1/10	0	9/100	81/100

Similarly, while 2.2 and 3.2, being alternative outcomes of the same procedure, cannot both confirm *h*, it is none the less possible that 2 as a whole and 3 as a whole should both do so, though not equally, if the confirming by 3.1 outweighs the disconfirming by 3.2 ; and likewise it is possible that 4 as a whole and 5 as a whole should both confirm *h*, though not equally.

In other words, although the finding that something, already known to be black, is a raven, is not in itself as good a confirmation of *h* as the finding that something, already known to be a raven, is black, yet the total observation of a black raven confirms *h* equally well in whichever order the characteristics are noticed. For we have to take account also of the *first* stage in each case, the finding that this previously uncharacterised object is black, and the finding that this previously uncharacterised object is a raven, and these first stages will also have some confirmatory effect, and a different effect from one another, in relation to whatever background knowledge we assume. In the same way, it is true that if finding that something, already known to be black, is a raven, confirms *h*, then finding that something, already known to be black, is a non-raven, cannot confirm *h* but must disconfirm it ; but it may be true none the less, when we take account of the confirmatory effect of the common first stage, the finding that this previously uncharacterised object is black, that the total observation of a black non-raven confirms *h*, though not so well as the total observation of a black raven.

The Alternative Outcome Principle, then, is itself sound, and its legitimate consequences can be reconciled with the Principle of Relevance. But the extreme view, that only those procedures can confirm a hypothesis which endanger it in the strong sense that one of their possible outcomes is a falsification, must be rejected. It is not

Consequently the reports in stages, 1.1, 1.2, 2.1, and 2.2 will have the following probabilities relatively to *k* and *k.h* respectively

	1.1	1.2	2.1	2.2
(i)	1/10	1/9	1/10	1/9
(ii)	1/10	1	19/100	10/19

Thus the addition of *h* raises the probability of 1.2 in the ratio 9:1, while it raises the probability of 2.2 only in the ratio 90:19, but it raises the probability of 2.1 in the ratio 19:10, while it leaves the probability of 1.1 unchanged. Multiplying the ratios for 1.1 and 1.2, and again for 2.1 and 2.2, we find that the total probabilities of 1 and 2 are raised equally in the ratio 9:1. Of course, when the probabilities are thus based on numerical distributions the answer has to come out right. But the example shows how this can be combined with a marked superiority of 1.2 to 2.2.

required by the Alternative Outcome Principle, and it conflicts with the Principle of Relevance. Indeed we can decisively refute this extreme view by comparing our procedures 1 and 2. Here we have two procedures whose total effects are equivalent ; but one of them contains a first stage which does not endanger h and a second stage which does endanger h, whereas the other contains two stages neither of which endangers h. Two procedures, then, which must be equivalent in confirmatory power, may fail to contain the same element of endangering ; it follows that endangering, in this strong sense, cannot be essential to confirmation.

4

I have said that the fact that a certain kind of observation would, relatively to limited background knowledge, confirm a certain hypothesis, does not imply that a sensible way of supporting it would be to try to make observations of that kind. I want to develop this hint and to link it with the claim constantly made by Professor Popper and his followers that a hypothesis is confirmed by a (favourable) observation only if that is made in a *test* of the hypothesis, in an attempt to falsify it.

This concept of testing, of attempting to falsify, must not be a psychological one, or this claim would fall foul of the Principle of Relevance. When challenged (by Mr Stove) to state an objective, non-psychological criterion of a *test* Mr Watkins referred to what I have been calling the Inverse Principle, saying that b is a genuine test of h if $p(b, k \,.\, h) > p(b, k)$; a test is a procedure in which a certain outcome is less probable without the hypothesis than with it. This is, I think, a fair general criterion for what is relevant to confirmation, but it lets in far more than the procedures that anyone would ordinarily count as tests or attempts to falsify in actual scientific practice. As we saw in section 2 above, this criterion would allow casual observations of black ravens—and even (to a slight degree) of non-black non-ravens—to count as tests of h in relation to the kind of background knowledge assumed in Alexander's solution of the paradox. But we can give a narrower (but still objective) account of a *test*. We may start from the concept of *looking for* or *trying to find* something. Ordinarily, no doubt, this concept is partly psychological, but it is not wholly so. It has an objective part, which can be considered on its own ; in this respect to look for, say, a lost golf ball is to adopt a procedure which *raises our chances* of coming upon the ball as compared with the procedure of,

say, sitting still or wandering aimlessly about. Similarly, to look for black ravens is to adopt a procedure which raises our chances of coming upon black ravens (if there are any) as compared with the procedure of, say, observing anything and everything that happens along. To test a hypothesis, I suggest, in the sense that we require, is analogously to look for a falsification of it, to adopt a procedure which raises our chances of falsifying it, if it is false. Let us see how *tests* and *looking for* various things, thus objectively defined, may affect confirmation.

Suppose, first, that we add, to the background knowledge assumed in Alexander's solution in section 2 above, the fact that we are looking for black ravens. Then this will raise the probability of b_1 reports above the previously assigned value xy. It will also raise the probability of b_1 reports in relation to k plus h above its previously assigned value x, but not in as high a ratio. Therefore the effect of adding to k the fact that we are looking for black ravens is to reduce the degree in which the adding of h increases the probability of b_1 reports, and hence to make such reports confirm h less well. And the more efficiently we look, the more is this so. In other words, as Popper's supporters (among others) have always said, if we try to confirm a hypothesis the favourable observations confirm it less well. This would be nonsense if 'try to confirm' had here a purely psychological meaning, but it makes very good sense if it means 'adopt a procedure which raises the chance of a favourable observation'.

The obverse policy, of not looking for falsifications, has an even more blatant effect. If we very efficiently do not look for non-black ravens, we may reduce to zero our chance of observing a non-black raven, even if there are some. And then the probabilities of reports of the forms b_1, b_2, b_3, b_4, given this policy and Alexander's background knowledge, will be x, 0, $y-x$, and $1-y$ respectively, that is, exactly the same values as when we add the hypothesis h. It follows that, given this policy, h does not raise the probability of any form of observation, and so no observation can confirm h if this policy is pursued with perfect efficiency.

On the other hand, if we (in this sense) try to falsify h, and look for non-black ravens, then we are adopting a policy which in relation to k alone raises our chances of observing non-black ravens, and therefore reduces the probabilities of the three other forms of observation, but which in relation to $k.h$ makes no difference, since if there are no non-black ravens our chances of finding one cannot be raised, however

hard we look. Thus by trying to falsify the hypothesis we create conditions in which the addition of *h* increases the probability of favourable observation-reports more than it would otherwise ; we create conditions in which the observation of a black raven is a better confirmation of *h* than it would otherwise be.

Although we cannot give numerical measures to these probabilities, it is plausible to say that in ordinary contexts the probability of a certain sort of observation, and hence the degree of confirmation of a hypothesis by an observation of that sort, will be radically affected by the observer's policy, by what he is looking for. Hence, if we restrict the notion of a genuine test more narrowly than Watkins (as quoted above) did, so that we count as tests only procedures which raise considerably the chance of falsifying the hypothesis if it is false, we may well be right in saying that in ordinary contexts worthwhile confirmations come only from genuine tests. On the other hand it will not be true that observations not made in tests do not confirm at all ; this is true only if ' test ' is given the wider definition that Watkins suggests, when it is simply a re-statement of the Inverse Principle.

This discussion supports Popper's doctrine with regard to actual investigation in ordinary contexts, but at the same time it shows that this does not conflict with something like an instantiation theory of confirmation in restricted contexts. The view that instances of black ravens confirm *h* in relation to severely limited background knowledge does not imply that we can confirm *h* by *looking for and finding* such instances (for such looking takes us beyond the limits set), and so it does not imply that it would be sensible to try to confirm *h* by looking for black ravens. And since this is so, we cannot argue from the fact that this is (obviously) not sensible to the rejection of that view.

5

The paradox of the ravens, then, is to be solved in different ways in different contexts.

(i) Given no additional knowledge, it is to be solved in Hempel's way ; in this context, proposition (2) in the set that constitutes the paradox can be denied without qualms.

(ii) Given only the information that ravens are less numerous than non-ravens, and black things than non-black things (and therefore that ravens are less numerous than non-black things), the paradox is to be solved in Alexander's way (or Hosiasson-Lindenbaum's, or Good's) ;

proposition (2) can be denied, but its plausibility is explained by the fact that we mistake a relatively poor confirmation for no confirmation at all.

(iii) Given unrestricted admission of relevant information of kinds that we commonly have, while proposition (1) is in general true and proposition (2) is in general false, they are so only if we take account of all confirmations, including quite weak ones, and it is more to the point to replace both (1) and (2) with (1′) and (2′) as follows :

(1′) Observations of black ravens confirm ' All ravens are black ' to a worthwhile degree only if they are made in genuine tests of this hypothesis (as defined in section 4).

(2′) Observations of black non-ravens never confirm ' All ravens are black ' to a worthwhile degree, and observations of non-black non-ravens confirm ' All ravens are black ' to a worthwhile degree only if they are made in genuine tests of this hypothesis.

It is clear that (1′) and (2′) are together compatible with (3).

This does not mean, however, that we cannot have a single theory of confirmation which applies in contexts of all sorts. In developing solutions of type (ii) we appeal to some theory that involves the Inverse Principle, and this principle is at least the basic idea of Popper's theory of confirmation, which gives us a solution of type (iii). The difference between these types of solution lies not in the fundamental principle that is used, but in the range of information called upon to supply the probabilities from which that principle generates confirmations. Type (ii) solutions draw their probabilities from relative distributions of characteristics, type (iii) solutions draw theirs from relevant information of all sorts, and the probabilities to which they refer can therefore be radically affected by the policies of the investigator himself. If there is a break in continuity, then, it comes not between solutions of types (ii) and (iii), but between those of types (i) and (ii).

But even a type (i) solution can be related to the Inverse Principle. For we might say that in an absolutely bare setting, to which this solution applies, reports of all the four kinds b_1, b_2, b_3, and b_4 are initially equally probable. Adding h in this context merely rules out the possibility of b_2 reports, and therefore increases equally the probabilities of reports of the other three kinds, that is, of all reports which are consistent with or satisfy h. Thus Hempel's satisfaction criterion of confirmation can be understood as the consequence of applying the Inverse Principle to the only probabilities available in a setting as bare as this.

I conclude, then, that a theory of confirmation based on the Inverse Principle can do justice to what the rival schools of thought have said about this paradox. The conflicts between these schools, on issues so far raised, can be resolved. They have arisen partly because the ' rival ' theories and ' rival ' solutions of the paradox have been interpreted as applying beyond their proper contexts, and partly because the Alternative Outcome Principle has been used over-enthusiastically and without a sufficiently precise specification of procedures and outcomes. The only legitimate dispute, if the Inverse Principle is itself accepted, would be about the probabilities to which it is applied ; for clearly a confirmation in terms of this principle can be no more objective than the probabilities from which it is generated.

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EACH AND ALL*

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CURRENTLY accepted theories of the foundations of mathematical analysis, or mathematics of continuity, and the modern mathematical theories of measure and Lebesgue integration, are expounded in terms of the theory of point sets originated by Georg Cantor (1845-1918). Since contradictions ('paradoxes') began to accumulate, much of the theory of infinite sets has been open to doubt. Of course, it does not follow at this stage that any of the findings of set theory are untrue. During the century and a half from Newton and Leibniz to Cauchy the fact that the calculus as then expounded was logical nonsense[1] did not prevent it from giving correct answers. And it is possible to believe that the earth is flat while arriving at correct conclusions on a great many issues.

However, in the case of set theory, the paradoxes persist in spite of the most strenuous efforts to remove them, and whether or not set theory gives correct answers is a matter of opinion. Two opposing points of view, one held by Brouwer and Weyl, the other by Gödel, are thus described by Weyl.[2]

> . . . a field of possibilities open into infinity has been mistaken for a closed realm of things existing in themselves. As Brouwer pointed out, this is a fallacy, the Fall and Original sin of set theory, even if no paradoxes result from it. . . . In a recent appraisal of Russell's contribution to mathematical logic he (Gödel) says that the paradoxes reveal 'the amazing fact that our logical intuitions are self-contradictory'. I confess that in this respect I remain steadfastly on the side of Brouwer, who blames the paradoxes not on some transcendental logical intuition which deceives us but on an error inadvertently committed in the passage from finite to infinite sets.

What Weyl had in mind is the 'actual infinite' or transfinite of Georg Cantor. A simple example that illustrates the kind of reasoning condemned by Weyl and Brouwer is the following tale concerning a race between two contestants I shall name Quick and Slowbutsure.

* Received 20.ii.62.

[1] As was made clear by Berkeley in his *Analyst* (1734).

[2] H. Weyl, *Philosophy of Mathematics and Natural Science*, Princeton, 1949, p. 234

The race is supposed to last for ever and is held on a straight track extending to infinity. Further, each contestant continues to run at a constant speed with Quick running twelve times as fast as Slowbutsure.

The application of Cantor's methods of reasoning about the infinite leads to the following analysis.

(1) Slowbutsure travels an inch while Quick travels a foot.

(2) The series of inches and feet has no last term.

(3) When Quick arrives at the *n*th foot, Slowbutsure has reached the *n*th inch; and the *n*th foot, being the 12*n*th inch, is reached by Slowbutsure when Quick is at the 12*n*th foot.

(4) Any selected (given, chosen, assigned, specified) [1] foot mark is the *n*th for a suitable value of *n*.

(5) Hence any given foot mark reached by Quick will be reached by Slowbutsure.

(6) Hence all foot marks reached by Quick will be reached by Slowbutsure.

(7) Hence the whole distance run by Quick will be traversed by Slowbutsure.

Our logical intuition protests that Slowbutsure will cover only one-twelfth of the distance run by Quick no matter how long the race lasts, and that, as the distance run by Quick gets ever larger, the gap between Quick and Slowbutsure gets ever larger and the full distance run by Quick—a distance open to infinity—is never attained by Slowbutsure. This 'intuitive' or 'commonsense' view is clearly opposed to conclusions (6) and (7) above.

Perhaps it would be well to assert that the conclusions of the analysis in steps (1) to (7) are not the result of an attempt on my part to be humorous. It is the fact that the same kind of reasoning is used in those parts of modern mathematics that accept Cantor's actual infinite as a legitimate logical concept. I refer the doubting reader to Bertrand Russell's discussion [2] of the paradox of Tristram Shandy.

Treating, then, steps (1) to (7) with the respect due to mathematics, can we resolve the conflict between conclusions (6) and (7) and the commonsense view? I believe the answer is yes and that the job can be done by exposing a fallacy in the Cantorian analysis.

Steps (1) to (3) are correct. Step (4) is correct only if a given foot

[1] In this context, the terms in parentheses are to be regarded as strictly synonymous with 'selected'. Justification for this is set forth in an Appendix.

[2] Bertrand Russell, *The Principles of Mathematics*, 2nd edn., London and New York, 1937, pp. 358–60

mark is taken to mean a (one) fixed specified foot in contrast to a variable foot. For example, after Quick has run 12 feet and is 11 feet ahead of Slowbutsure, then that (variable) foot mark specified as the one closest to 10 feet behind Quick is not the nth for a suitable value of n. While an understanding of this point is helpful in understanding the nature of the fallacy, let us agree to overlook it for the present. We assume, then, that 'any given foot mark' means any one specified fixed foot mark. With this assumption as to the meaning of (4), step (5) is correct.

But (6) is false. A correct consequence of (5) is

(6*a*) Hence all foot marks that are describable by a suitable value of n will be reached by Slowbutsure.

But as Slowbutsure approaches infinity, or at infinity if we admit Cantor's actual infinite, there will be foot marks beyond him that are not specifiable fixed foot marks. (There are, of course, specifiable *variable* foot marks such as that one nearest to 10 feet beyond Slowbutsure.)

In brief, every member of an infinity cannot be specified else there would not be an infinity. It is in this sense that what is true of each is not necessarily true of all. The counter assumption that what is true of any given fixed element is true of all is the fallacy of the actual infinite.

Also, it will now be clear that if 'any given foot mark' in step (4) is allowed to refer to a variable foot, then (4) to (7) are almost trivially false.

Interesting results can be obtained by using the same logical theme and varying the correspondences. For example, if step (3) is changed to

(3′) The $2n$th foot is reached by Slowbutsure when Quick is at the $24n$th foot; and if (4) stays the same, then (5) becomes

(5′) Hence Slowbutsure reaches a point twice as far as any given foot mark reached by Quick; and (6) becomes

(6′) Hence Slowbutsure attains twice the distance of all points reached by Quick.

The next example I have chosen to illustrate the distinction between each and all is a very simple one. Consider the sequence of numbers of the form $1/n$, where n is a positive integer. To avoid misunderstanding, I exclude as a matter of definition the number 0 from this sequence, which will be denoted by $\{1/n\}$.

It is easily seen that for any given fixed positive number p, no matter how small, there are numbers belonging to the sequence $\{1/n\}$

that are less than p (because if n is large enough, $1/n$ will be less than the fixed p). Hence, if what is true of each is true of all, there are numbers of the set $\{1/n\}$ less than all numbers p greater than 0. But the only numbers less than all numbers greater than 0 are 0 itself and negative numbers. Hence there are elements of the sequence $\{1/n\}$ that are 0 or negative, which is contrary to hypothesis. In this case, therefore, what is true of each cannot be true of all.

More complex and more controversial than the preceding example is the analysis on the basis of each and all of the principles involved in Cantor's diagonal process. This process is famous in modern pure mathematics, and mathematicians are divided into believers and heretics according to whether or not they have faith in it.

Cantor used the diagonal process to prove that the numbers between 0 and 1 cannot be enumerated, that is, cannot be arranged in the natural order of the integers. I first outline, without comment, Cantor's proof as it appears in E. W. Hobson's *The Theory of Functions of a Real Variable and the Theory of Fourier's Series.*

Assume the contrary, that the numbers between 0 and 1 are enumerable. If so, said Cantor, they can all be arranged in order in a list such as the one exhibited, where each number is represented as an infinite decimal fraction. (If a number happens to equal a finite decimal fraction, such as $\frac{1}{2} = 0{\cdot}5$, we consider it to be followed by an infinity of zeros.)

$$
\begin{aligned}
&. a_{11}a_{12}a_{13}a_{14} \ldots \\
&. a_{21}a_{22}a_{23}a_{24} \ldots \\
&. a_{31}a_{32}a_{33}a_{34} \ldots \\
&\qquad \ldots \\
&\qquad \ldots \\
&. a_{n1}a_{n2}a_{n3}a_{n4} \ldots \\
&\qquad \ldots
\end{aligned}
$$

The a_{ij} represent digits from 0, 1, 2, 3, 4, 5, 6, 7, 8, 9.

Cantor then defines a new number $. b_1b_2b_3b_4b_5 \ldots$ by means of the rule that the digit b_1 is to differ from a_{11}, b_2 is to differ from a_{22}, b_3 from a_{33} and, in general, b_n is to differ from a_{nn}. This new number differs from any in the above list, since it differs from the first number in the list in the first decimal place, from the second in the list in the second decimal place, etc. And this new number is a number between 0 and 1. Thus it is impossible to arrange all numbers between 0 and 1 in order since it is possible to define at least one that is not included in the list.

Let us examine the argument in detail. If an infinite set is enumerable, this means that any given fixed element of the set can be assigned an integer distinct from the integer assigned to any other given fixed element of the set. The assigned integer, of course, fixes the position of the element in the natural order of the integers. *But enumerability has nothing to say about assigning integers to all the elements of an infinite set.* When it is said that enumerability implies that *all* the elements can be arranged in a list, this is not literally true. If it were, it would mean that an infinite process has an end, a contradiction in terms. Purely as a manner of speaking it may be regarded as all right to say that the elements of an infinite enumerable set can all be arranged in order, but in dealing with the elusive infinite one cannot be too careful. However, we may as well accept Cantor's reference to arranging all the numbers in order to be purely a way of speaking because the same point of logic occurs in his definition of the new number . $b_1b_2b_3b_4$. . . . This time Cantor's treatment allows of no equivocation.

The proof depends on the assumption that the new number, which we shall denote by B, is defined. Now B is defined if and only if all of its digits, the b_n, are defined. There is no doubt that for any given fixed n the digit a_{nn} would be defined and consequently the digit b_n could be defined. But when Cantor claims that a new number B is defined by this procedure, he is relying on the assumption that what is true of each is true of all. (This assumption was later given the name of the axiom of choice.) B cannot be regarded as defined unless the infinite process involved in its attempted definition can be completed, at least in principle. But in principle no less than in an actual operation, the completion of an infinite process is a contradiction in terms. The essence of Cantor's diagonal process is that an infinite set is considered as no more than a collection of fixed specifiable individuals. But any such collection is finite.

The fact that the diagonal process is logically unsound does not, of course, necessarily invalidate all or any conclusions obtained by its use. I shall return to the particular conclusion of Cantor that the numbers between 0 and 1 are unenumerable. Meantime the following simpler way of looking at the diagonal process may be helpful.

There are two infinite processes involved. The first is the arranging of all the numbers in order, the second is the choice of a b_n which can only take place when the value of the a_{nn} becomes known. Cantor says, in effect, ' Complete your set of a_{nn}'s then I'll complete my set

of b_n's and produce the new number B'. But the set of a_{nn}'s has no end so the set of b_n's is not complete so the method does not define a particular number B. It is similar to a case of generalship that General b_n would win if only General a_{nn} would fully commit himself. But General a_{nn}'s nature is such that he never fully commits himself and the result is inconclusive. This viewpoint makes it clear that nothing can be proved or disproved by the diagonal process.

As for the truth or falsity of Cantor's result that the numbers between 0 and 1 are unenumerable, it was proved by T. Skolem in 1923 that such numbers are indeed enumerable. As far as I know, Skolem's proof has not been refuted. As far as I can gather, most mathematicians refuse to believe what Skolem's theorem says because they believe in the validity of Cantor's diagonal process.

I now take up my fourth and last example of the difficulty of each and all. This is the proof that the rational numbers between 0 and 1 can be enclosed in a set of intervals of total length as small as we please. As in the case of the diagonal process, I first give the proof without comment.

It is well known that the rational numbers are enumerable. This means there is a first rational, a second rational, etc. Enclose the first rational in an interval of length ϵ where ϵ is less than $\frac{1}{2}$, the second rational in an interval of length ϵ^2, the third in an interval of length ϵ^3, and so on. Then the total length of all the intervals equals the sum of a geometric series and remains less than 2ϵ. Since ϵ is arbitrary, the total length may be made as small as we please.

Let us now critically examine this result and the reasoning behind it. Every mathematician believes that if *all* points on the line from 0 to 1 are enclosed in intervals, the total length of such intervals cannot be less than 1. Hence, if it be true that intervals containing all the rational points have a total length less than 1, there are points not included in those intervals. These would, of course, be the irrational points. Let P denote one such point.

Every mathematician believes that the rationals are everywhere dense, in the following sense. There are rational numbers (points) closer to any point Q than any given fixed non-zero number, no matter how small. *Yet in the case of the point P, the proof under examination claims to include ALL the rationals in given non-zero lengths none of which reach P.*

This leads to the question of who or what goes to infinity first. In the diagonal process the infinity involved in defining the new number B cannot be completed until the infinity of the a_{nn} is complete. Since

the latter is not attained, the proof is inconclusive. In the case we are now discussing, one infinity is complete, that of the rationals, not because we have achieved the impossible and completed it but because all the rationals are present by hypothesis. For ages, aeons, before the proof under discussion was devised, the nature of things had already gone to one limit, as it were, and produced *all* the rationals. There they wait, ahead of time, closer to every P than any choice that can be made. The belief that selected intervals can be assigned to all the rationals so that P remains excluded is a delusion.

From the proof and the above analysis of the result, a definite conclusion can be drawn, namely, the one-by-one or step-by-step procedure of the proof does not, *even in principle*, attain all the rationals even though they are enumerable. Somewhere, 'back of beyond', the rationals close in more thickly than the sequential approach can (ever) achieve. The fallacy in the proof is the assumption that because any given fixed rational can be assigned a selected fixed interval this can be done for all rationals.

The case seems to be different if we consider only the integers. Although it is literally nonsense to speak of all the integers being attained, it appears a relatively harmless manner of speaking to say that all the integral points can be enclosed in intervals whose total length may be made as small as we please. The reason is that the integers are nowhere dense and therefore it is possible to assign non-overlapping intervals to them.

Of course the integers, by definition, are enumerable. The fact that the procedure of the proof when applied to the integers results in no great harm but yields an incorrect answer when applied to the enumerable rationals implies that the property of enumerability is not a very distinctive one, that by itself it is far from sufficient to characterise an infinite set of points. This observation may be regarded as a corollary of Skolem's theorem of 1923 which states that any formal system has an enumerable model.

Some Tentative Criteria for Permitting the Extension from Each to All

To conclude this essay, I suggest the following as sufficient conditions for the assumption that what is true of each is true of all.

(1) If the set is finite.

(2) When 'all' is not an actual or completed infinity—when there is no connotation of arriving at the end of an infinity, a contradiction in terms. For example, consider the set of numbers, excluding 0,

of the form $1/n$, where n is a positive integer. Each element of this set differs from 0, hence all elements of the set differ from 0. Here, the statement that all the elements are greater than 0 is almost a rephrasing of the definition of the set $\{1/n\}$ and has the same logical standing as, for example, the following: each element of a set is a positive integer, hence all elements of the set are positive integers.

The notion of a completed infinite always yields inconclusive results when there is, expressed or implied, a second completed infinite dependent on the first. An example is the Cantor diagonal process where the completion of the b_n depends on the completion of the a_{nn}.

The notion of a completed infinite is always vicious, that is, always yields wrong results, when an infinity existing ahead of time is considered not yet complete in order that an otherwise impossible dependent choice leading to a second infinity may be made. For example, if P is an irrational point, then for any given fixed rational R it is possible and permissible to choose an ϵ_R interval containing R but excluding P, but it is neither possible in practice nor permissible in logic to achieve this for all rationals R. This has been discussed in the fourth example of this essay. In the same category is the second example I considered where, for any given fixed p greater than 0, there are elements of the sequence $\{1/n\}$ less than p, but there are no elements of $\{1/n\}$ less than all p greater than 0. Here, the infinity of all numbers greater than 0 exists ahead of time, hence the choice of the second infinity, that of those elements of $\{1/n\}$ less than all p greater than 0, cannot be made.

(3) When requirements originating outside abstract set theory in the particular nature of the set elements being considered indicate that the notion of a completed infinity is merely a harmless manner of speaking.

This is the case with a convergent sequence. While no number of the sequence $\{2 + 1/n\}$, $n = 1,2,3, \ldots$ equals 2, the sequence may be regarded as specifying the number 2 just as if infinity were attained. In general, a convergent sequence cannot be completed and in strict logic cannot be (define) a number, but the requirement of continuity of the number system demands the existence of a limiting number for every convergent sequence. This is why it is harmless, *except within logic itself*, to consider that an infinite sequence is logically complete and defines its limit.

Let us return to an illustrative example stated in terms of each and all. Suppose we know that X represents an undetermined but fixed non-negative number. Further, suppose we have proved that X is

less than ϵ for any given fixed ϵ greater than 0. Then it indeed follows that X is less than all ϵ greater than 0 and that X equals 0.

The crux of the matter is that the X, though unknown, is fixed and its choice is not dependent on our treatment of the infinity of numbers ϵ that are greater than 0. Once this is satisfied, the notion of an infinity of choices of epsilons greater than zero is harmless. We can prove that X is less than the given numbers $\epsilon_1 > \epsilon_2 > \epsilon_3 > \ldots > \epsilon_n > 0$ for n finite but as large as we wish. Then the $\{\epsilon_n\}$ is a convergent sequence and it is harmless to consider that we can reach 0 via such a sequence. It is just as if we could indeed complete the infinite choice of ϵ_n's.

Appendix (an elaboration of Footnote 3).

I use these several terms to help explain the connotation I intend. This cannot be defined except by the context of this essay and it is indeed the purpose of this essay to make my connotation clear. I believe that Clerk Maxwell once made fun of Herbert Spencer for stating that it is important to keep the meaning of a term somewhat vague. In the case of a recognised technical term I am sure we would agree with Maxwell that the term should be defined as exactly as possible. But if Spencer had in mind the investigation of a phenomenon not conforming to traditional ideas and theories, where the full nature of the phenomenon is as yet unknown, then exact definition is only possible at the end of our investigation. And, perhaps, in using interim terms to describe what we understand only vaguely, it may be an advantage not to try to define them too closely. If that is what Spencer had in mind, I would find myself in strong sympathy with his point of view. Now that physics has its uncertainty principle it may be possible for scientists to be a little more patient with Spencer than Maxwell was. And it is now possible to quote de Broglie, 'Is it not a general fact that when our ideas are formulated somewhat vaguely they are in the main applicable to reality, while when we attempt to make them absolutely precise they become ideal forms whose real content has vanished?' (taken from 'Réalité physique et idéalisation' in *Revue de Synthèse*, 1934).

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RELIABILITY OF INFORMANTS*

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1. *Introduction*

AN analysis will be proposed for two explicanda, viz. two intuitive concepts of reliability well-known in the methodology of the criticism of historical documents.

The following desiderata are expected to be satisfied by the explicata:

(1) It should prove useful for the methodology of history, in particular in reconstructing the theorems presupposed in methodological rules such as the one allowing us to accept as probable a statement independently asserted by reliable authors, or the rule according to which the fact that a statement h is asserted by an author N does not confirm h, if N is unreliable.[1]

(2) It should prove useful for the general methodology of science in formulating rules for the acceptance of observation statements and in formulating rules in terms of which a 'well-tested universal hypothesis' could be defined. Some philosophers of science insist that the verdict 'this hypothesis has been well-tested (confirmed)' should be based on a critical appraisal of the whole testing procedure taking into account the experimenter's competence, his sincerity and self-criticism, the possibilities at his disposal, etc., all these are elements in the appraisal of the reliability of historical documents as practised by historians.[2]

Keeping these desiderata in mind we will refer to reliability of informants in general, meaning thereby not only authors of historical documents, but also observers, witnesses, experimenters testing hypotheses and the like.

To avoid some likely misunderstandings the following comments may be helpful.

* Received 11.ii.62.

[1] Examples of such rules, explicitly formulated, may be found in M. Handelsman's *Historyka* (Warsawa, 1928).

[2] The relation between confirmation and sincerity in testing a hypothesis and therefore also reliability was stressed by K. R. Popper in *The Logic of Scientific Discovery* (London, 1959), p. 418.

The paper is not intended to be a presentation or reconstruction of the entire procedure of the criticism and interpretation of historical documents.

Methodological rules analysed here apply to the procedure of testing hypotheses against one type of historical source, viz. documents intended by their authors to communicate information. In the opinion of contemporary historians sources of this type constitute only a small fraction of the whole evidence at their disposal and the proportion of *reliable* documents is, of course, even smaller. This is, however, no drawback from our point of view. For we are not concerned here with a complete reconstruction of the entire procedure of testing historical hypotheses or with the appraisal of the limits and reaches of this procedure. Rather a fragment of the historian's activity is examined in the hope of showing that this activity and that of scholars working in other fields are subject to the same laws.

In what follows, first the explicatum is given and then some of its applications are discussed.

2. *Two explicanda: reliability$_1$ and reliability$_2$*

At least two intuitive concepts of reliability henceforth referred to as reliability$_1$ and reliability$_2$ are known in the methodology of the criticism and interpretation of historical documents.

Reliability$_1$ is referred to whenever historians affirm that most of the items of information in a given domain communicated by a certain author N turned out to be true (distortions of information were rare) and there were very few failures to report important items of information in the given domain, therefore N is reliable in the domain. Conversely, if an informant N frequently distorts information or abstains from communicating important items in the given domain, then N is said to be unreliable in the given domain. Reliability$_1$ may, therefore, be defined as the frequency of false and true items of information in a domain.[1]

Reliability$_2$ is involved when we refuse to believe in the assertions of an informant N because we know that N did not sincerely try to test the given item of information, or that N was unable to test it on account either of his incompetence or of external obstacles of any kind (e.g. lack of technical means, social or political pressure, etc.), or that N was under the influence of an erroneous doctrine while

[1] 'Information' is used here in the neutral sense of a statement affirmed by an informant.

acquiring and communicating the information. Thus reliability$_2$ may be said to be a matter of the aims and means possessed by the informant, or of the logic of the situation in which he acted, or—which amounts to the same—of his rational cognitive and informative behaviour. In this sense an informant is reliable if he consciously intended to acquire the information in question and to communicate it and was in possession of adequate means to achieve both these aims.

Instead of distinguishing the above two concepts of reliability we may, if we wish, distinguish two methods of testing the hypothesis that an informant N is reliable. The first of the methods serves to appraise the reliability of a certain informant on the basis of a series of statements asserted by him, assuming that we know the truth-value of these statements. The second method enables us to appraise the reliability of the informant on the basis of our knowledge of his intentions or aims and of the situation in which he acquired and communicated the information. The second method does not presuppose any assumption as to the truth-value of the statements asserted by the informant, which is not unimportant if we appreciate the fact that one of the functions of the appraisal of reliability is to assist us in the decision whether or not to accept an item of information communicated by the informant.

The former of our two explicanda and likewise the former of the two methods refer to assertive behaviour of the informant, to his asserting (believed to be) true or false statements in the given domain. The latter explicandum and likewise the second method refer to the informant's 'latent structure', viz. to his intentions, competence, skill, knowledge, etc., and to the situation in which he acted.[1] In some cases it is the truth-value of the asserted statements that is assumed by the critic as given to him, in others again it is the latent structure of the informant and his situational logic.

The two intuitive concepts of reliability are interesting for the following reasons:

(*a*) Reliability$_1$ seems definable in terms of statistical probability, provided we make the concept of the reliability domain more precise,

[1] The concept 'latent structure' was borrowed from P. F. Lazarsfeld, 'A Conceptual Introduction to Latent Structure Analysis', in *Mathematical Thinking in the Social Sciences*, ed. P. F. Lazarsfeld (Glencoe, 1955).

For the concepts of situational logic and rational behaviour see K. R. Popper, *The Open Society and its Enemies* (London, 1957). Cf. also K. R. Popper's idea of probability as a property of generating conditions. The 'Propensity Interpretation of Probability', this *Journal*, 1959, 10, 25.

which we will do in the next paragraph. A definition of reliability in terms of probabilities is desirable to satisfy our *desideratum:* (1) likewise to satisfy, (2) as it would facilitate comparisons of the rules of the criticism of historical documents with the methodological rules of other fields of research. Furthermore, some elements of reliability are dispositional in character, e.g. competence. Dispositional concepts of this kind are usually defined in terms of probabilities of behaviour of a certain type.

(*b*) Reliability$_2$ is related to the concept of rational behaviour, viz. it may be understood as rational cognitive and informative behaviour. However, the end at which this behaviour aims may be defined in terms of frequency of true statements, i.e. in terms of reliability$_1$. The situational logic of a reliable informant is, therefore, reconstructible with the help of the following, nonempirical theorem:

> *If the cognitive and informative aims are not dominated by any other incompatible aim in the preference scale of an informant N, and N has sufficient means at his disposal to achieve his aims, then N achieves his aims.*

If ' cognitive and informative aims ' is defined in terms of high frequency of true statements in a given domain and ' sufficient means ' is taken with the *ceteris paribus* proviso, then a reliable informant in the sense of reliability$_2$ satisfies the antecedent of the above theorem and achieves, in accordance with it, his cognitive and informative aims in terms of a high frequency of true recorded statements, which makes him reliable in the sense of reliability$_1$.

3. *The explication of reliability*

We are going to explicate the concept of reliability[1] by reconstructing one of the possible procedures of appraising an informant's reliability. It will be a so-called ' rational reconstruction ' and not a description of any actual case.

In our reconstruction we shall characterise not only the informant's but also the critic's behaviour. As models of the critic we may consider historians appraising the reliability of the author of a document, scientists appraising the results of testing a universal hypothesis by another experimenter, etc.

In the present paper, which is a preliminary to a more extensive study of reliability, we shall confine ourselves to discussing situations

[1] For the sake of simplicity we shall define a pair of classificatory concepts: 'reliable', 'unreliable'; 'reliability' in historian's terminology is rather a relative term.

which involve only one critic and one informant. In actual practice, however, information is usually transmitted by several informants and their reliability is appraised by a group of critics, who—like any social group—have their own aims, norms of behaviour, habits, etc., even when their interests are in conflict. A theory of reliability would have to include a theory of monopoly, duopoly, oligopoly on both sides—the informant's and the critic's; furthermore, it would have to deal with a hierarchy of informants (indirect transmission of information) and of critics, i.e. a critic's appraising the reliability of another critic's appraisal of an informant's reliability. The concept of independence (of informants and of critics) is, of course, essential for such a theory. It seems that more interesting results for the philosophy of science and normative methodology may be expected from the outlined extension of the present study.

The following auxiliary concepts appear in the reconstruction of the procedure in which one critic appraises the reliability of one informant:

the reliability domain D_k	$p_1\bar{p}_1$	$p_2\bar{p}_2$	$p_3\bar{p}_3$	$p_4\bar{p}_4$	$p_5\bar{p}_5$	$p_6\bar{p}_6$	$\cdots p_n\bar{p}_n$
the ideal informant's law of behaviour	*aa*	*am*	*ma*	*am*	*am*	*am*	$\ldots aa$
the sequence of the informant's interpreted utterances	p_1	$\bar{p}_2$	$\bar{p}_3$	—	—	p_6	$\cdots p_n$
the critic's hypothetical knowledge of the truth value of $p_t \epsilon D_k$	*VF*	*FV*	*FV*	*VF*	*FV*	*VF*	$\ldots FV$
the line of the informant's assertive behaviour in D_k	I_1	I_2	I_3	O_4^m	I_5	I_6	$\ldots O_n^d$

The reliability domain constructed by the critic and denoted by D_k is a set of pairs of contradictory historical statements[1], which form alternative answers to questions of interest to the critic, e.g. to questions referring to a temporal sequence of events such as the outbreak of a revolution, etc. The pairs of statements are assumed to be logically independent; further conditions would have to be imposed on the set of course, to form a normative concept of a rational reliability domain. In the simplest case the reliability domain, as conceived here, is a set of pairs of logically independent basic statements.[2]

The law of behaviour of the ideal informant determines the assertive behaviour of the informant, ideal from the critic's point of view, in the given reliability domain. Two modes of behaviour are allowed:

[1] By a historical statement is meant here any statement involving spatial-temporal determinations.

[2] 'Basic statements' as defined in K. R. Popper's *The Logic of Scientific Discovery*, Chapter V.

assertion of a statement belonging to D_k, which is denoted by the letter *a;* assertion or abstention from mentioning a statement in D_k, which is denoted by the letter *m*. Notation *aa* is to be read: of two contradictory statements $p_i, \bar{p}_i$ in D_k assert the true one; *am* means: assert p if true, assert or omit $\bar{p}$ if p is false; *ma* is, of course, the reverse of *am*. The ideal informant is defined as the one that strictly conforms to the law. The combination *mm* is not theoretically excluded; however, it will not appear in our notation since it would mean that the critic is not interested in either p or $\bar{p}$ and will rather refrain from including this pair of statements into the reliability domain. The point of the proposed notation is the following: we expect a well-informed informant to affirm the occurrence of a fact important in the given domain but do not expect him to deny its occurrence if the non-occurrence is ' in the nature of things '. Let, for the sake of illustration, the fact in question be the eclipse of the sun interfering in some human affairs; it would not be reasonable to expect any informant reporting on any sequence of events in the history of mankind to deny the occurrence of an eclipse and its interference in the course of events, unless there had been a prediction or a rumour which we consider it the informant's duty to have known and to have corrected.

The letters *V*, *F* do not stand, as is usual, for truth and falsity, but for the critic's belief in the truth or falsity of a given statement belonging to the reliability domain.[1]

The sequence of the informant's utterances interpreted by the critic is written with the same type of symbols that were used in writing the reliability domain. This is to emphasise that the sequence thus written is the result of the critic's interpretation (translation) of the original utterances of the informant by mapping them on the reliability domain. The original utterances may differ from the language of the reliability domain in point of style or may be in an altogether different language. The sign ,—, occurs whenever no statement in the original sequence of utterances corresponds to a statement p in D_k.

In the informant's behaviour line the symbol 1 denotes the conformity of the informant's assertive (interpreted) behaviour with the ideal informant's law; O^m stands for the informant's failure to assert a tatement in D_k to which an ' *a* ' corresponds in the law of the ideal informant's behaviour; O^d stands for the denial of a (believed to be) true statement in D_k.

[1] *V*, *F* might also stand for ' probable (improbable) given *H* ', where *H* is a statement accepted by the critic.

Furthermore there appear in our definition: the constant α and the concept of random (nonsystematic) errors. α is a small number chosen by the critic as the upper limit of the frequency of errors of a reliable informant. This limit may be referred to as the level of criticism characteristic of the given appraisal of reliability. It may be varied by the critic for various reliability domains. If the critic wants to replace the classificatory concept of reliability with a relative one, he construes a scale of frequencies of zeros (errors) instead of just fixing the limit α.

Randomness of zeros (errors) in the informant's behaviour line is explained in terms of the following procedure: the critic construes a list of tests of reliability$_2$ of the informant N in D_k. These are conjectures that N does not satisfy some at least of the conditions of reliability$_2$ in a subdomain of D_k, e.g.: N was not competent in the subdomain of economic information included in D_k; N was a nationalist and did not intend to communicate true political information included in D_k; N did not have sufficient means for the acquisition of technical information included in D_k etc. On the basis of a definite list of such tests construed by the critic, subdomains of D_k are selected, e.g. statements communicating economic, political, technical etc. information. Then the frequency of zeros is counted for each subdomain. If the informant's behaviour line is insensitive to any selection listed, i.e. if the frequency of zeros in any subdomain selected according to the list does not exceed the limit α, then zeros (errors) in the behaviour line are said to be random, otherwise there are systematic errors in the behaviour line.[1]

With the help of the above auxiliary concepts we construct the definition of a reliable informant:

Df[1]: If a long behaviour line of N is given in D_k, then N is reliable in D_k if and only if:

(1) $F(O) < \alpha$

(2) zeros in the behaviour line are random.

Condition (1) of the above definition requires the frequency of errors and omissions (failures to assert) not to exceed the limit α, which requirement is a straightforward explication of the intuitive concept of reliability$_1$ demanding that a reliable informant shall not distort information too frequently and that he be informative.

[1] The procedure of testing randomness or errors presented here is an adaptation of the procedure for constructing random sequences given by K. R. Popper in *The Logic of Scientific Discovery*, Chapter VIII.

Condition (2) of the definition is but rarely satisfied. Therefore its satisfaction is to be treated rather as a marginal case. Relative reliability may be determined in terms of deviations from the marginal case, by specifying the subdomains in which the frequency of zeros exceeds the limit α. There seems to be no obstacle in differentiating limits for various subdomains, which would, of course, require a modification of condition (2).

The method of testing the randomness of errors in the behaviour line of the informant is relativised to a given list of tests construed by the critic. Errors (lies, omissions) which the critic does not want or cannot show to be systematic with the help of suitably construed tests, are classified as random (nonsystematic). Should this subjective approach to randomness be unacceptable, an alternative objective approach may be suggested: for this purpose it suffices to regard the tests listed by the critic as tests of the hypothesis that the informant's errors are random in some absolute (objective) sense. In this case it is only the method of testing randomness, i.e. the choice of tests, that is relativised to the critic, as is any method of testing any hypothesis. Tests on the list are attempts at refuting the randomness hypothesis. The appraisal of the informant's reliability depends anyway on the critic's ingenuity, competence, and possibilities at hand, therefore the list of tests of randomness of errors constructed by the critic may be regarded as the level of criticism of the appraisal.

The definition is a reductive one: it is limited to the cases in which we are given a long behaviour line of the informant in D_k. It does not apply without modifications to informants who have not made any utterances or very few ones. The principle 'silence is go den' does not govern the appraisal of reliability as presented here.

The definition was arrived at by reconstructing the procedure of appraising the reliability of an informant. The empirical content of the hypothesis: 'N is α-reliable in D_k' is to be determined in terms of the reliability domain D_k, of the law of the ideal informant, of the critic's assignment of truth values to any $p_i \epsilon D_k$ and of tests of randomness. No wonder then that it should be possible to show analogies between these concepts and the concept of the critical region in statistical terminology.[1]

[1] Cf. the present author's note 'Confirmation, Critical Region and Empirical Content of Hypotheses', *Studia Logica*, 1960, 10, 122-5.

4. *Some applications and problems*

A. *Confirmation rules.* Several confirmation rules appealed to by historians will be given in what follows. These rules involve our explicatum, therefore it may be claimed that historians behave as if they accepted our definition of reliability.

A simple notation will be used (on the left hand side) and likewise a translation given (on the right hand side). $Rel(N,D_k)$ stands for 'N is reliable in D_k'; as (N,b) for: 'N asserts b'; D_k^a denotes the subset of D_k to which all those statements belong that are distinguished by an 'a' in the law of the ideal informant (the set of important statements included in D_k).

Rule (1).

$Rel(N,D_k)$	N is reliable in D_k
as (N,b)	N has asserted b
$b \epsilon D_k$	b is a member of D_k^a
$Rel(N,D_k) \rightarrow P\,[\text{as } (N,b),b] \simeq 1$	If N is reliable in D_k, then the probability given b, of N's asserting b is great.
b	b may be accepted (or: b has been confirmed).

The above rule (or scheme) is a combination of the *modus ponens* with weakened reduction,[1] viz. on the basis of the first premiss by *modus ponens* we detach the consequent of the fourth premiss; the latter in conjunction with the second and third premisses yield the conclusion by virtue of the weakened reduction rule, which allows us to accept tentatively (as confirmed) a hypothesis if an event has occurred very probable on this hypothesis. By *modus ponens* we detach one of the consequences of the definition of reliability. As the conclusion was reached by (weakened) reduction we may say that the conjecture that b is true is a hypothetical explanation of the fact that a reliable informant has asserted a statement b belonging to D_k.[2]

The above rule is, however, not applicable to any element of D_k, but only to the elements of the subset D_k^a. The reason is that reliability domains differ with respect to the subsets D_k^m and D_k^a. If the ratio of

[1] For 'weakened reduction' and other weakened schemes see Z. Czerwiński, 'On the Relation of Statistical Inference to Traditional Induction and Deduction', *Studia Logica*, 1958, 7, 243–62.

[2] Strictly speaking we should say 'weakened explanation'.

the number of statements in D_k^m to those in D_k^a is great, the fourth premiss might not be true.

The following rule is applicable to N's asserting any statements in D_k:

Rule (2):

$Rel(N,D_k)$	N is reliable in D_k
$as(N,\bar{b})$	N has denied b
$b \epsilon D_k$	b is an element of D_k
$P[as(N,\bar{b}),b] \simeq 0$	the probability, given b, of N's denying a statement in D_k is very small
$\bar{b}$	Therefore: reject b (or: b has been undermined).

The above rule (scheme) is a variant of the weakened *modus tollendo tollens*: a hypothesis b is rejected, or is considered undermined, if an event has occurred, viz. the denial of a statement in D_k by a reliable informant, the probability of which given b is very small.

Rule (3):

$Rel(N,D_k)$	N is reliable in D_k
$b \epsilon D_k^a$	b is an element of D_k^a
$P[as(N,b),b] \simeq 1$	the probability, given b, of N's asserting b is very great.
$as(N,\bar{b})$	N has denied b
$\bar{b}$	Therefore: reject b (or: b has been undermined).

Rule (4):

$Rel(N,D_k)$	N is reliable in D_k
$b \epsilon D_k^a$	b is an element of D_k^a
$P[as(N,b),b] \simeq 1$	the probability, given b, of N's asserting b is very great
$\bar{b} \epsilon D_k^m$	*non-b* is an element of D_k^m
$\bar{as}(N,b) \,.\, \bar{as}(N,\bar{b})$	N has refrained from mentioning b (has neither asserted nor denied b)
$\bar{b}$	Therefore: reject b (or: b has been undermined).

The above rule is a case of the so called *argumentum ex silentio*; a reliable informant's abstention from asserting the occurrence of an event, which if it occurred would belong to important facts in the given domain, is the basis of the rejection of the hypothesis that the event has occurred.

Rules (1)-(4) are rarely applied without an appeal to independent evidence, either in the form of testimonies of independent informants or otherwise. The following rule makes reference to independent informants:

Rule (5):

$Rel(N,D_k) \,.\, Rel(M,D_k)$	N and M are reliable in D_k
$as(N,\bar{b}) \,.\, as(M,\bar{b}) \,.\, b \epsilon D_k$	N and M have denied b and b belongs to D_k
$P[as(N,\bar{b}),b] \simeq 0$	the probability, given b, of N's denying b, is very small,
$P[as(M,\bar{b}),b] \simeq 0$	the probability, given b, of M's denying b is very small,
$P({}_N O \,.\, {}_M O) = P({}_N O) \,.\, P({}_M O)$	N's and M's errors in D_k are mutually independent.
$P[as(N,\bar{b}) \,.\, as(M,\bar{b}),b] < P[as(N,\bar{b}),b]$	the probability, given b, of N's and M's denying b is smaller than the probability of N's denying b.
$\bar{b}$	therefore: reject b (or: b has been undermined).

In the above rule, which may be generalised to an arbitrary number of informants, the concept of independence occurs essentially. Historians frequently appeal to the theorem that with a growing number of conforming testimonies of independent and reliable informants the probability of error decreases and becomes insignificant.[1] Naturally, if probability is here meant in the statistical sense, then independence is meant in the same sense. The behaviour lines of our informants N and M are statistically dependent on each other, because they depend on the same reality which they describe. Otherwise N and M would not be reliable. However, all that is necessary for the rule is independence of errors, viz. that M make errors with the same frequency when N errs as when N does not (or: does not more frequently); or at least that these two proportions do not differ significantly. This is stated in the fifth premiss of the above scheme.[2]

If M errs more frequently when N does than otherwise, we are entitled to suspect either that M knew N's report (or *vice versa*) when writing his and simply relied on it instead of on reality, or that M's and N's errors had a common source and thus were systematic in the same way. The latter possibility is eliminated if N and M are reliable in the sense of our definition, as any systematic errors are excluded thereby. The former may be eliminated by appropriate tests, e.g. by

[1] Cf. M. Bloch, *Apologie pour l'histoire* (Paris, 1953), p. 145

[2] It seems in conformity with the historian's intuition to assume that the sets of errors of N and M in D_k are independent.

showing that M's report was written before N's was, or that M did not understand the language in which N's report was written or that there are striking dissimilarities in style, viewpoint, etc. between both reports.

Rule (5) may be applied to the *argumentum ex silentio* of several informants.

B. *Some confirmation rules referring to unreliable informants.* Our definition of reliability implies that if an informant N is unreliable in D_k, the frequency of his errors is not small (is greater than the limit *alpha*) or his errors are not random but systematic. This consequence of the definition is the basis of the following principles:

(1) the first principle of indetermination: If an informant N is unreliable in D_k and N asserts h belonging to D_k, then these two items of information are not sufficient for a decision to accept or to reject h (alternatively: to regard h as confirmed).

(2) the second principle of indetermination: If N is unreliable in D_k and N neither asserts nor denies h (there being only three alternative modes of behaviour) and h is an element of D_k, then these items of information are not sufficient for a decision whether or not to reject h or *non-h ex silentio* (whether to regard h or *non-h* as disconfirmed *ex silentio*).

(3) the third principle of indetermination: If N is unreliable in D_k and we (the critic) are in possession of independent evidence for the acceptance of h belonging to D_k, then these items of information are not sufficient for the prediction whether or not N would assert h or deny it.

Condition (2) of our definition implies that errors of an unreliable informant are systematic, i.e. determined by some discovered factors. If we know these factors we can predict and explain the errors.

(4) the principle of determination of errors of an unreliable informant: If h has been well-confirmed, h is an element of D_k and has been denied by N who is unreliable in D_k, there exists an explanation of N's behaviour either in terms of a statistical or in terms of a strict law.[1]

(5) the principle of translation: If N is unreliable in D_k and the critic k is able to map N's errors, i.e. zeros in N's behaviour line, on a

[1] There are, of course, random errors in the unreliable informant's behaviour line too. However, explanation in terms of a single coincidence is unsatisfactory.

set Z of possible explanatory statements, then an error O_i of N's confirms a statement z_j, where $z_j \in Z$, if z_j is part of a satisfactory explanation of O_i.

(5′) translation by negation: In a special case, the set Z of explanatory statements consists of negations of the sentences asserted by the informant and classified as O_d by the critic.

(6) the principle of resolving an apparent incompatibility by explanation: If a hypothesis h is incompatible with our well-tested knowledge W, thus $P/h,W/ = O,h$ is an element of D_k, N asserts h and N is unreliable in D_k in the sense that N has the property f such that for every x: if $f(x)$ then the probability given *non-h* of x's asserting h is not small, then the fact of N's asserting h is not incompatible (does not disconfirm) with *non-h* or W.

The first three of the above principles refer to situations in which we merely know that the informant is unreliable, but do not know the law of his unreliability (the law of his unreliable behaviour). The principles (4)-(6), on the other hand presuppose that the factor determining unreliability is given to the critic. If a reliable informant affirms a statement which is an element of his reliability domain, then the best explanation of his assertive behaviour is the conjecture that h is true. However, the same conjecture would not be a good explanation if the informant is unreliable. In the latter case we should rather explain his behaviour in terms of a statement z belonging to a set Z, if there exists a rule R that maps Z onto the behaviour line of the informant. In a special case the statement z in Z may be the negation of a corresponding h in the informant's behaviour line. The statement z would in conjunction with other statements form the 'situational logic' of the unreliable informant's behaviour.[1]

C. *The concept of historical source for a given question.* Apart from the unrelativised concept of historical sources which appears in the classifications such as that proposed by Droysen, Bernheim, and others,[2] there is in the historian's terminology a relativised concept: it is mentioned when we say that something is or is not a source of information relative to a given question, e.g. that the testimony of an unreliable informant asserting a statement h is not a source relative to the question whether or not h (is true), but may be a source relative to a different question whether or not some z is true.

[1] We assume that the probability of the *explicandum* given the *explicans* should be great in a good or satisfactory explanation.

[2] Cf. E. Bernheim, *Lehrbuch der Geschichtlichen Methode* (Leipzig, 1930).

The following definitions, in terms of the confirmation rules given in the preceding paragraph, are proposed:

> Df^2: An interpretation of the form: 'N affirms at time t and place l that p' is a source relative to the question q if and only if there exists a hypothesis h proposed as a solution of the problem q and a rule R such that the interpretation either confirms or disconfirms h by virtue of R.
>
> Df^3: An object x is a source (of information) relative to the question q if and only if there exist observation statements $S_1 \ldots S_n$ referring to observable properties of x, a hypothesis h proposed as a solution of q and a rule R such that $S_1 \ldots S_n$ either confirm or disconfirm h by virtue of R.

D. *Systematic omissions.* An appraisal of the informant's reliability in the sense of Df^1 takes into account not only the frequency of errors and of lies, which might be reflected in a coefficient of errors and lies, but also the frequency of omissions of relevant facts (failures to affirm the occurrence of relevant facts).

We have appealed to what may be called the coefficient of omissions when construing the *ex silentio* rule. That informativeness in the sense of a small coefficient of omissions is closely related with the intuitive concept of reliability may be gathered from the fact that in many countries informants in the witness box have to swear 'to speak the truth, the whole truth and nothing but the truth'. Reference to 'the whole truth' is understandable only under the condition that it is known which facts are relevant; this condition is incorporated in the idea of reliability domain D_k.

The distinction between the error and the omission coefficients is helpful in clarifying what is meant by saying that an author gave a thoroughly false description of a situation or of a sequence of events without affirming one single false statement.

E. *Reliability in testing a universal hypothesis.* In the recent controversy over inductive logic and confirmation K. R. Popper insisted that the concepts of confirmation, of a well-tested (corroborated) hypothesis etc. cannot be adequately defined in terms of probabilities because our intuition requires that the verdict: 'this hypothesis has been well-confirmed' should be the result of an appraisal of the whole testing procedure and that such factors as the experimenter's sincerity in testing, his competence and the means at his disposal ought to be

taken into account in the appraisal.[1] Neither is it possible to replace this somewhat ethical requirement of sincerity with the proviso that to really corroborate a hypothesis H the testing procedure must not fail to apply all tests of H available on the existing knowledge; there would be an infinite number of such tests and so no hypothesis would ever be corroborated. An attempt at reducing the number of these tests by discriminating between relevant (or interesting) and irrelevant (uninteresting) ones may be successful only if we relativise 'relevant', e.g. if we relativise it to a critic or to critics. A similar relativisation was necessary in the appraisal of reliability and since sincerity, competence, technical possibilities, etc., are all factors considered in the appraisal of reliability, the concepts proposed in Section 3 of this paper may be applicable in reconstructing both procedures.

Let the reliability domain D_k be interpreted as the set of test-statements, i.e. of pairs of basic statements and their negations, which in the opinion of the critic k ought to be applied in testing a universal hypothesis H; let the informant be an experimenter testing H, the sequence of his utterances—his report of the results of tests (positive or negative for H); let the law of the ideal informant's behaviour determine the behaviour of an experimenter ideal in the critic's opinion. $F(O^d)$ will then be the coefficient of errors in reporting the results of tests of H, $F(O^m)$ the coefficient of failures to apply relevant tests. The testing of H and the appraisal of the results of testing H may then be regarded as a model of our reconstruction of the behaviour of the informant and of the critic appraising his reliability.

A considerable coefficient of omissions, $F(O^m)$ may be due either to the deficiency in the experimenter's technical equipment, or to his incompetence and lack of ingenuity or to the insincerity of his attempts to overthrow the tested hypothesis. It seems that any significant difference between the 'inductivist' and the 'critical approach' to testing one and the same hypothesis should be reflected in the structure of D_k, D_k^s, D_k and in the law of the ideal informant's behaviour.[2]

[1] K. R. Popper, *The Logic of Scientific Discovery*, p. 418.

[2] As an example of a severe test of an informant's reliability we may perhaps consider a 'test in a difficult (or dangerous) situation'; it may be argued that if the informant managed to acquire and to transfer information in a situation difficult or dangerous for him, he may safely be relied on in a 'normal' situation. A similar idea of a severe test seems to underly the suggestion that 'friends are identified only in emergency situations' or that extreme situations are the touchstone of people's morality.

The reference to the controversy over inductivism reminds us that we may use the proposed concepts not only for descriptive but also for normative methodological purposes. An inductivist and a deductivist will presumably have different ideas of a 'good', 'rational' reliability domain, suitable for testing an informant's reliability. A rational reliability domain will be defined in terms of special conditions to be imposed on the set of statements forming the reliability domain. The construction of this set must not be left to the arbitrary decision of the critic as critics differ in interests, competence, sincerity etc., just as do informants. A critic might construe the reliability domain so that the examined informant is sure to be reliable in it; as a matter of fact D_k may be construed *ad hoc*, i.e. not to test the informant's reliability but to show at any cost *that* the informant is reliable. If we left the construction of the reliability domain to a critic's discretion without any restrictions and entered the game as his passive partners, the game might turn out to be degenerate, a game whose matrix allows dictatorial strategies, i.e. one in which we would always lose against a ruthless critic.[1]

Two critics, say k_1 and k_2, will not differ significantly in their appraisal of an informant's reliability if:

(1) D_{k1} and D_{k2} are similar, i.e. if their difference is sufficiently small (does not exceed a limit),

(2) D^a_{k1} and D^a_{k2}, D^m_{k1} and D^m_{k2} are likewise similar in the sense explained above,

(3) the assignments of values V, F to statements in D_{k1} and in D_{k2} do not differ significantly.

However, if more than two critics are involved, the above conditions do not suffice, as the relation 'similar' is not transitive. It would be necessary to require that the symmetrical difference between any two reliability domains does not exceed the given limit.

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[1] The concepts 'improper (degenerate) game' and 'dictatorial strategy' were proposed by the present author in 'A Generalization of the Refutability Postulate' *Studia Logica*, 1960, **10**, 97-108.

NOTE AND COMMENT

The Refutability of 'Irrefutable' Laws

As is well known, Popper has characterised scientific statements as being empirically refutable. Now there are some statements that *may* not be *known* to be refutable, such as those of general relativity had there happened to be no moon and no planet Mercury; such statements, which are in fact refutable, may be thought to be irrefutable but *cannot* be *known* to be. A statement once irrefutable in the then state of knowledge can become refutable when someone thinks of a test for it. There is, therefore, a distinction between statements for which no possible refutation has been proposed and those for which a method of refutation can be specified.

Consider those statements for which no possible refutation has been proposed. Some of them cannot be known to be irrefutable, and we might over-readily suppose that all are in this position. But this is not so; some are irrefutable in principle. Since there are examples of these that form part of science, they apparently run counter to Popper's characterisation of science. They are of major importance also in other ways, as will appear below.

An example from physiology is: 'All bodily changes are due to physical causes.'[1] When a bodily change occurs for which no physical cause is known, this is not accepted as a refutation—rightly because for there to be a counter-example there would have to *be* no physical cause, and this is not established by no physical cause being *known*. It is characteristic, as Popper[2] first showed, that an infinitely wide (in space) and long (in time) search for the required physical cause can be carried on: predictions consist of purely existential statements which, as Watkins[3] has put it, assert the existence of something that is neither circumscribed nor localised by any boundary. Since these might be verified but could never be falsified, the law in its turn might be verified but could never be falsified.

Similarly in psychology, the doctrine of epiphenomenalism, 'All mental changes are due to physiological causes',[4] also allows an infinitely open field of search for confirmations.

[1] J. O. Wisdom, 'Psycho-Analytic Technology', this *Journal*, 1956, **7**, 25

[2] K. R. Popper, *The Logic of Scientific Discovery*, London, 1959, p. 70

[3] J. W. N. Watkins, 'Confirmable and Influential Metaphysics', *Mind*, 1958, **67**, 345-7

[4] J. O. Wisdom, 'Is Epiphenomenalism Refutable?', *Proc. 2nd Inter. Congr. Philos. Sc.*, Zürich, 1954

Likewise in physics, the law of conservation of energy can never be falsified because of the possible existence of unknown forms of energy that would confirm it. The law began in the form that the sum of the kinetic and potential energy is constant. Then heat energy had to be included, and various other additions, such as electrical energy, had to be made to preserve the constancy. Whenever energy seems to be lost, it is possible that it will be found in a new form. This possibility leaves the field of confirmation infinitely open, and hence the law is irrefutable.

The first comment to be made is that a specific form of the law is refutable, and several have been refuted. When one is refuted it is replaced by a new form. All that survives is the unspecific form.

It is hard to conceive what a situation would be like in which energy was lost in some process and not transformed in some new guise. But, so long as it is not found, a research physicist has to decide whether or not to go on looking for a new form, and if there should be no new form he has no means of finding out, however long he may keep on with unsuccessful attempts, that there is none. This shows that the unspecific form of the law could be confirmed, if a new form happened to be discovered, but could never be known definitely to be false. Thus the unspecific form of the law is irrefutable. When a principle, or something resembling a refutable hypothesis, has this character, it has, as I have previously put it,[1] a largely programmatic function—whatever other function it may also have—it prescribes a programme of research. Thus the unspecific principle of conservation of energy, if a specific form is refuted, tells you to look for a new form. But it goes further, for it prescribes limits to the kinds of theory you should try to construct: thus it has the function, as Watkins has put it,[2] of a regulative principle, which governs not phenomena but theories, or constitutes a framework that theories must fit.

If a framework is not refutable, can it be given up only because of weariness, disillusionment, or failure, or can it be given up on rational grounds ? I will propose a refutability theorem that would enable this to be done.

I will use yet another example. A long-standing assumption in physics was that ' Energy occurs in all possible quantities (or is continuous) '; this is clearly infinitely open to confirmation in the same way as before, and is irrefutable. Now Planck discovered phenomena of radiation which he interpreted as due to discontinuous processes (quanta of action). It would be difficult to interpret the phenomena otherwise, but in principle it was possible that this could be done if ever other quantities of action should be found that would restore continuity.

[1] J. O. Wisdom, ' Psycho-Analytic Technology ', this *Journal*, 1956, **7**, 26

[2] J. W. N. Watkins, ' Between Analytic and Empirical ', *Philosophy*, 1957, **32**, 130

However, Schrödinger's discovery of the wave-equation constituted a theory with discontinuities as consequences. This theory was independently testable (i.e. testable otherwise than by means of quanta of action), tested, and confirmed (e.g. by the spectrum of the hydrogen atom). Now it is a deductive consequence of the wave-equation that energy-levels are discontinuous. Hence Schrödinger's theory refutes the assumption that energy is continuous.

Similarly a theory with perpetual motion as a consequence would, if tested and confirmed, refute the unspecific law of conservation of energy.

It is vital to note that when the above examples were said to be irrefutable, this meant that no fact or observation could refute them,[1] but that they may be refuted in a different way, namely by an independently tested and confirmed theory. Thus a framework, which governs not phenomena but theories, may be refuted by theories.

It should be added that this kind of refutation is hypothetical in a way that refutation by observation is not, for the refuting theory, though tested and confirmed, may later be falsified; then the programme it had refuted becomes 'derefuted', and reverts to its original position of being infinitely open to confirmation but never to refutation by further search for appropriate facts.

So long as a tested theory is not refuted, one has no option until it is seriously challenged but to accept its observational consequences (e.g. the prediction of eclipses) and its consequential empirical generalisations (e.g. the harmonic motion of a pendulum): no sensible or rational person would try to build a simple clock and deliberately act against the law of harmonic motion. One has to accept equally any other sort of consequence of such a theory, and therefore to reject any observationally irrefutable law that it refutes.[2]

It would seem that a major breakthrough in science is brought about, not by the refutation of a theory at the hands of an observation or the replacement of the theory by another one, important though this is, but by the

[1] What was basically referred to in the context of refutability of empirical hypotheses was refutability by observation; Popper is indeed concerned with refutability by any method, but the core of his fundamental contribution to the theory of what constitutes science lies in refutability by observation.

[2] Watkins and Bartley have been concerned with the clash between a regulative principle and a testable empirical theory (J. W. N. Watkins, 'Confirmable and Influential Metaphysics', *Mind*, 1958, **67**, 345; W. W. Bartley III, *The Retreat to Commitment*, New York, 1962, pp. 85, 159). Watkins was mainly concerned in the context with the opposition to an empirical theory by a regulative principle (and with the position of a regulative principle that follows from an empirical theory); I am concerned with the converse, i.e. an empirical theory opposing a regulative principle. Bartley touched on this; he recognised the direction of this opposition; but he was concerned with *disproof* and rightly noted that a regulative principle cannot be *disproved* by an empirical theory, and thus did not extract the idea of refutation in the sense I have put forward.

refutation of a framework at the hands of a theory and its replacement by the framework of that theory.

Such refutability has, further, a significant bearing on what is to be considered as rational. The examples given of observationally irrefutable laws in physiology and psychology exercise strong pressure to regard a purely psychological theory and the theory of psychosomatic medicine as unscientific and irrational, because they are incompatible with the laws mentioned which form part of reputable branches of science and which derive prestige from being irrefutable.

To summarise the thesis: A law that can be confirmed without risk of failure, because what is sought to confirm it may sometimes be found but never be known to be non-existent, is 'irrefutable' in the sense that there is no observation that can refute it; if a theory is discovered that is testable without assuming the falsity of the law, actually tested, and confirmed; then the law is (hypothetically) refuted by the theory.

J. O. WISDOM

DISCUSSIONS

Corroboration and Rules of Acceptance

According to Popper, '. . . the doctrine that *degree of corroboration or acceptability cannot be a probability* is one of the most interesting findings in the philosophy of knowledge.'[1] Unfortunately, the debate that raged a few years ago between Popper and the apologists for Carnap made the significance of Popper's thesis depend upon a determination of who had explicated what presystematic concept adequately.[2] Taking Popper's 'finding' to be a gambit in a version of 'button, button, who has the button' hardly makes it 'interesting'. The philosophical importance (if any) of Popper's thesis should be evaluated in terms of the success with which Popper can apply his measure of corroboration to the problems he designed this measure to handle and the philosophical significance of these problems.

Popper's writings offer ample evidence of his intention to formulate rules for accepting and rejecting scientific hypotheses with the aid of his corroboration measures:

> Furthermore, the degree of confirmation is supposed to have an influence upon the question whether we should *accept*, or choose, a certain hypothesis x, if only tentatively; a high degree of confirmation is supposed to characterise a hypothesis as 'good' (or 'acceptable'), while a disconfirmed hypothesis is supposed to be 'bad'.[3]

This passage not only indicates that Popper considers degree of corroboration relevant to formulating acceptance rules but suggests a general acceptance rule:

> Given a set of mutually incompatible hypotheses choose that hypothesis with the maximum degree of corroboration relative to the available evidence.

This rule confers genuine significance on Popper's claim that corroboration is not a probability. He is rejecting any acceptance rules which recommend maximising probability. (For the duration, we shall assume that probabilities are logical measures à la Jeffreys and Carnap.) He does this without denying relevance to probabilities in formulating rules of acceptance; for degree of corroboration is a function of probability.

Obviously the dispute between Carnapians and Popperites has been over the wrong issue. Carnap nowhere claims that scientists maximise probabilities when accepting hypotheses. In point of fact, he denies that scientists accept or reject hypotheses at all.[4] He recognises only two applications of his 'degree of confirmation':[5]

[1] K. R. Popper, *The Logic of Scientific Discovery*, London, 1959, p. 394

[2] See the notes by Popper, Carnap and Bar-Hillel in this *Journal*, 1955, 6, 155–63, and 1956–57, 7, 243–56

[3] K. R. Popper, *The Logic of Scientific Discovery*, p. 399

[4] Carnap, 'Remarks on Induction and Truth', *Philosophy and Phenomenological Research*, 1945–46, 597–8

[5] R. Carnap, *Logical Foundations of Probability*, Chicago, 1950, pp. 241–79

(1) Assignments of degrees to which evidence confirms (in his sense) hypotheses.

(2) Provision of rules for deciding how to act relative to practical objectives. If a man is confronted with alternative courses of action and knows the utilities relative to his goals of adopting any one of these policies given that any one of a set of exclusive hypotheses is true, Carnap says that the decision maker ought to maximise expected utility.

Thus, the issue separating Carnap from Popper is not whether scientists do or ought to maximise probability when accepting hypotheses but whether they accept and reject hypotheses at all.[1]

For the remainder of this discussion, Popper's position on this matter will be dogmatically adopted. Our objective is not to question whether scientists accept and reject hypotheses but whether in doing so they do or ought to maximise probability, corroboration, or anything else.

Imagine a hypothetical Carnap who admits that scientists accept and reject hypotheses in accordance with the principle that probabilities ought to be maximised. Such a person might attack Popper's view by turning the tables on Popper's celebrated refutation of Carnap.[2]

Consider a homogeneous die. With Popper let x be the statement 'six will turn up on the next toss' and z the statement 'an even number will turn up'. $-x$ is the negation of x. $P(x) = 1/6$, $P(-x) = 5/6$, and $P(z) = \frac{1}{2}$. The probability of x given z is $1/3$ and of $-x$ given z is $2/3$. Suppose that a rational gambler is constrained to place an even money bet (assumed equal to utility) on whether a six will or will not turn up on the next toss. The Bayes method (which recommends maximising expected utility) prescribes betting on $-x$ even if the gambler knows that z, which lowers the probability of $-x$, is true.

Suppose the gambler is not constrained to bet money but is asked to predict whether the next toss will turn up a six or not. He has a disinterested concern to predict correctly—i.e. he is indifferent as to whether a six or some other side turns up. We can say that he assigns equal 'epistemic' utilities to predicting x when x is true and $-x$ when $-x$ is true and equal utilities to predicting x when x is false and $-x$ when $-x$ is false. (Greater utilities are to be assigned to potential correct answers than to potential errors.) The Bayes method recommends accepting $-x$ on evidence y even though y undermines $-x$ in Popper's sense.

[1] Even if Popper had not explicitly stated that degrees of corroboration are to be used to generate acceptance rules, he would have had to do so in order to provide corroboration with philosophical significance. He could not claim that scientists merely assign degrees of corroboration to hypotheses. This view is no more plausible than Carnap's parallel view regarding the 'theoretical' applications of degree of confirmation. Nor could he claim that scientists are guidance counsellors and that degrees of corroboration provide a guide in life. Carnap could do this with logical probabilities; for there are decision theories extant that do provide a basis for using probabilities in determining practical policy. Whatever the merits of these theories, they at least are available for critical scrutiny. No theory is available for utilising corroboration in practical decision making. Hence, the significance of corroboration must stand or fall with its relevance to the formulation of scientific acceptance rules.

[2] For one version see Popper, op. cit., p. 390. His example is the one presented with modifications in the text.

Our hypothetical Carnapian might generalise from this sample and maintain that scientists always seek the truth and nothing but the truth in the sense that they always assign equal epistemic utilities to potential correct answers and equal but lower epistemic utilities to potential errors. The Bayes method would require that such truth seekers maximise probability; for the probability of x given y would be directly proportional to the expected epistemic utility of accepting x on evidence y.

This 'refutation' of Popper depends upon two important assumptions: (*a*) that scientists always seek the truth and nothing but the truth when accepting and rejecting hypotheses; and (*b*) that epistemic utilities can be assigned to potential correct and incorrect answers in accordance with von Neumann-Morgenstern postulates for rational preference or at the very least with postulates which justify maximising expected epistemic utilities.

Anyone familiar with Popper's writings knows he would reject assumption (*a*) outright. According to Popper, the scientific importance of a proposition varies with its falsifiability or logical content. Hence, only when the alternative hypotheses from which one is called upon to choose are equal in scientific importance, would Popper admit that the scientist seeks the truth and nothing but the truth. For the sake of the argument we shall agree that Popper is right.

Popper nowhere indicates clearly what stand he would take respecting assumption (*b*), but he does recognise the notion of scientific importance and attempts to measure it. Moreover, his measure of corroboration is a 'weighting' of scientific importance with probability. Now we can agree with Popper that when scientists accept and reject hypotheses they assign higher 'epistemic utility' to hypotheses with high content than to those with low content. We can also agree that scientists attempt to maximise some weighting of epistemic utility with probability when accepting hypotheses. But we can without contradiction reject the specific kind of weighting embodied in Popper's measure of corroboration. In order to justify maximising corroboration, we must supplement our agreements with additional assumptions concerning rational preference which together with our initial assumptions entail maximising corroboration. Unfortunately, Popper fails to offer any such postulates for preference. Instead, he presents 'desiderata' for adequate explications of some presystematic concept of corroboration.[1] Popper's various measures of corroboration undoubtedly

[1] The remainder of this discussion presupposes familiarity with the discussion in Popper, op. cit. pp. 265-81 and pp. 387-419. For purposes of our discussion, the following of Popper's definitions of $C(x, y)$—the degree of corroboration of x given y—and $E(x, y)$—the explanatory power of x relative to y—have been used:

$$C(x, y) = E(x, y)(1 + P(x, y)P(x)).$$

$$E(x, y) = \frac{P(y, x) - P(y)}{P(y, x) + P(y)} \text{ where } P(y) \neq 0.$$

Popper's conditions of adequacy or desiderata for 'degree of corroboration' are laid down on pp. 400-1. They are reproduced here except for (vi) which is quoted later in the text:

(i) $C(x, y) \gtreqless 0$ respectively if and only if y supports x, or is independent of x, or undermines x.

(ii) $-1 = C(\bar{y}, y) \leq C(x, y) \leq C(x, x) \leq 1$

(iii) $0 \leq C(x, x) = C(x) = 1 - P(x) \leq 1$

(iv) if y entails x, then $C(x, y) = C(x, x) = C(x)$

explicate the presystematic concept characterised by these desiderata. This fact, however, in no wise supports his thesis that we ought to maximise corroboration any more than one can justify maximising expected utility by showing that expected utilities satisfy the postulates of an algebra of expectations.

In the remainder of this paper, we shall adopt assumption (*b*), i.e. a theory of rational 'epistemic' preference based on von Neumann-Morgenstern type postulates (except that we shall use logical instead of frequency or subjective probabilities). Should Popper balk at this procedure, he is obligated to supply an alternative theory.

Let $P(x, y)$ be the conditional probability of x given y, $C(x, y)$ be the degree of corroboration of x given y, $u(x)$ the utility of accepting x when x is true and $u(x)'$ the utility of accepting x when x is false. Given assumption (*b*), justification of Popper's recommendation that we maximise corroboration requires that we make $C(x, y)$ equal to the expected epistemic utility of accepting x on evidence y:

$$(A)\quad P(x, y)u(x) + P(-x, y)u(x)' = C(x, y).$$

If (A) is tenable and scientists do not in general seek the truth and nothing but the truth, $C(x, y)$ obviously cannot be a probability. But is (A) tenable?

A negative answer to this question can be given if we can show that utility functions $u(x)$ and $u(x)'$ which satisfy (A) must violate von Neumann-Morgenstern postulates for preference or any alternative postulates that justify maximising expected utility. To this much we are committed by assumption (*b*).

Let us consider 'prizes' *f*, *g*, *h*, and *i* which are preferred in increasing order from left to right. Von Neumann and Morgenstern would assign numerical utilities to these prizes roughly as follows: First, assign numbers F and I to any pair of the prizes —say *f* and *i*—subject only to the restriction that the preferred prize *i* receive the higher number. Second find a gamble with *f* and *i* as prizes such that the probability of winning *f* is *p* and *i* is $1-p$ and such that the gambler is indifferent between taking this gamble or having *g* for certain. Compute the value G of $pF + (1 - p)I$. Finally, assign G as the utility of *g*. Repeat the procedure for *h*.

Among the postulates for preference that a person must satisfy before such utilities can be assigned to his preferences is that for any 'pure' alternative there is a gamble among other alternatives such that the gambler is indifferent as to having the gamble or the alternative Moreover, it must be assumed that his preferences for the prizes in the gamble have not changed in virtue of being incorporated in the gamble. *But this implies that it must be both possible and rationally legitimate for a person's preference patterns to remain unaltered even if the probabilities of attaining the various prizes change.* A person whose preferences for various prizes change with the chances of attaining them (as in the case of the horse-better who has a penchant for long shots) cannot have his preferences measured by von Neumann-Morgenstern procedures. A theory which requires that preferences change in this way is incompatible with von Neumann-Morgenstern postulates.

(v) if y entails $\bar{x}$, the $C(x, y) = C(\bar{y}, y) = -1$

(vii) if $C(x) = C(y) \neq 1$, then $C(x, u) \gtreqless C(y, w)$ whenever $P(x, u) \gtreqless P(y, w)$.

(viii) if x entails y, then: (*a*) $C(x, y) \geq 0$; (*b*) for any given x, $C(x, y)$ and $C(y)$ increase together; and (*c*) for any given y, $C(x, y)$ and $P(x)$ increase together.

(ix) If $\bar{x}$ is consistent and entails y, then: (*a*) $C(x, y) \leq 0$, (*b*) for any given x, $C(x, y)$ and $P(y)$ increase together; and (*c*) for any given y, $C(x, y)$ and $P(x)$ increase together.

Now the 'epistemic' utilities we are attempting to construct are designed to reflect the preferences that scientists ought to have for the various possible correct and incorrect answers available to them when they are called upon to choose among competing hypotheses. The probabilities we propose to use in constructing utility assignments are logical probabilities of the truth and falsity of the various hypotheses on the available evidence. We must do this if we are to use von Neumann-Morgenstern type postulates to justify maximising expected epistemic utility where expectation is a function of logical probabilities. If we made utility assignments to potentially correct answers and errors a function of the evidence—e.g. made the utility of accepting a hypothesis x when it is true vary with the available evidence y—then these utilities would vary with conditional logical probabilities of the hypotheses. Therefore, such utility assignments could not be made in conformity with von Neumann-Morgenstern type postulates. Consequently, we may lay down the following condition of adequacy for epistemic utility assignments:

(I) No rational or scientific assignment of epistemic utilities must require that utilities vary with the available evidence.

It can be shown that if (A) is adopted, the utility functions generated do violate (I). Hence, (A) is untenable on all available theories which support maximising expected utility. The outcome is that for the present, at any rate, there is no justification for Popper's contention that scientists ought to maximise corroboration *even if we concede to him that scientists do accept and reject hypotheses and that highly falsifiable propositions are scientifically more important than those with low content.*

The proof of this contention runs as follows:

According to (A): $\mathrm{P}(x, y)\mathrm{u}(x) + \mathrm{P}(-x, y)\mathrm{u}(x)' = \mathrm{C}(x, y)$

If $\mathrm{P}(x) \neq 0$, $\mathrm{C}(x, y) = \dfrac{\mathrm{P}(x, y) - \mathrm{P}(x)}{\mathrm{P}(x, y) + \mathrm{P}(x)}\,(1 + \mathrm{P}(x, y)\mathrm{P}(x))$

Let $\mathrm{P}(x, z) = \mathrm{P}(x)$. Then $\mathrm{C}(x, z) = 0$

Hence, $\mathrm{C}(x, y) = \mathrm{C}(x, y) - \mathrm{C}(x, z)$

$= (\mathrm{P}(x, y) - \mathrm{P}(x))\mathrm{u}(x) + (\mathrm{P}(-x, y) - \mathrm{P}(-x)\mathrm{u}(x)'$

$= (\mathrm{P}(x, y) - \mathrm{P}(x))(\mathrm{u}(x) - \mathrm{u}(x)')$

According to (I), $\mathrm{u}(x)$ and $\mathrm{u}(x)'$ and, hence $\mathrm{u}(x) - \mathrm{u}(x)' = k$ must remain constant through variations in y and $\mathrm{P}(x, y)$.

But $k(\mathrm{P}(x, y) - \mathrm{P}(x)) = \mathrm{C}(x, y) = \mathrm{P}(x, y) - \mathrm{P}(x)\,\dfrac{(1 + \mathrm{P}(x, y/\mathrm{P}(x))}{(\mathrm{P}(x, y) + \mathrm{P}(x))}$

Hence, $k = \dfrac{1 + \mathrm{P}(x, y)\mathrm{P}(x)}{\mathrm{P}(x, y) + \mathrm{P}(x)}$

Hence, the value of k must vary with $\mathrm{P}(x, y)$ counter to (I).

The situation is no different when $\mathrm{P}(x)$ and $\mathrm{P}(x, y)$ are both equal to 0. Here

$$\mathrm{u}(x)' = \mathrm{C}(x, y) = \mathrm{E}(x, y) = \frac{\mathrm{P}(x, y) - \mathrm{P}(y)}{\mathrm{P}(y, x) + \mathrm{P}(y)}$$

If we restrict ourselves to cases where $\mathrm{P}(x) \neq 0$, it is clear from the proof that the violation of (I) stems from the 'normalisation' of $\mathrm{P}(x, y) - \mathrm{P}(x)$ which yields $\mathrm{C}(x, y)$. Had $\mathrm{P}(x, y) - \mathrm{P}(x)$ been multiplied by a constant factor, the proof of the inadequacy of (A) would not have gone through. Let us choose unity as the factor and write $\mathrm{U}(x, y) = \mathrm{P}(x, y) - \mathrm{P}(x)$. We now propose to consider $\mathrm{U}(x, y)$ as the function to be maximised in place of $\mathrm{C}(x, y)$.

$P(x, y)u(x) + P(-x, y)u(x)' = U(x, y) = P(x, y) - P(x)$.
Let $y = x$. Then $P(x, y) = 0$, $u(x)' = -P(x)$.
Let $y = x$. Then $P(x, y) = 1$, $u(x) = P(-x)$.

Putting these values of $u(x)$ and $u(x)'$ back in our original equation:

$$P(x, y)P(-x) - P(-x, y)P(x) = U(x, y)$$

for constant x and variable y.

The utility assignments $P(-x)$ and $-P(x)$ have much to recommend them from Popper's point of view; for they rank the importance of hypotheses with high content higher than those with low content. Moreover, $U(x, y)$ can be identified with expected epistemic utility of accepting x on evidence y without violating von Neumann-Morgenstern type postulates. And, interestingly enough, almost all of Popper's desiderata for $C(x, y)$ are satisfied by $U(x, y)$. In conclusion, it might be useful to explore this matter somewhat further.

$U(x, y)$ satisfies all of Popper's desiderata save (ii), (v), and (vi). The violations of (ii) and (v) are minor and are due to the fact that $U(-x, x) = -P(-x)$. Since this violation will have no serious bearing on the hypotheses that are accepted on given evidence, we may ignore it and turn to (vi) which reads as follows:

> Let x have a high content—so that $C(x, y)$ approaches $E(x, y)$—and let y support x. (We may, for example, take y to be the total available empirical evidence.) *Then for any given* y, *C* (x, y) *increases with the power of* x *to explain more and more of the content of* y), and therefore with the *scientific interest* of x.[1]

Consider first the case where $P(x)$ and $P(z)$ have positive values very close to 0. (vi) recommends that we choose between x and z on the basis of their likelihoods $P(y, x)$ and $P(y, z)$. When at least one of the ratios $\frac{P(x, y)}{P(z, y)}$ and $\frac{P(x)}{P(z)}$ is very close to unity, maximising $U(x, y)$ approximates this recommendation very well. In other cases, the approximation is very poor. We may legitimately wonder, however, whether we ought to place the entire burden of our choice on likelihood when there are significant differences in content and conditional probability. Thus, if

$$P(x, y) = .0003,\ P(x) = .0001,\ P(z, y) = .002 \text{ and}$$
$$P(z) = .001,\ U(x, y)$$

will be one-fifth the value of $U(z, y)$ whereas the likelihood of x relative to y will be one and one-half times as large as that of z relative to y and the ratio of $C(x, y)$ to $C(z, y)$ will approximate that very closely. It is not clear that presystematic judgments so strongly recommend choosing x over z that we would be warranted in introducing a desideratum which entails this result.

When the contents of x and z are exactly unity and their conditional probabilities 0 (the typical case with universal hypotheses), $U(x, y) = U(z, y) = 0$, whereas $E(x, y)$ and $E(z, y)$ will frequently have different positive values. Hence, $C(x, y)$ enables us to choose between hypotheses in important cases where $U(x, y)$ fails utterly. In general, the use of the Bayes method with epistemic utilities when choosing among universal hypotheses will result in identifying the expected utility of accepting a universal hypothesis with the utility of accepting such a hypothesis when it is false no matter how we assign these utilities.

[1] Ibid., p. 401

This consequence of using the Bayes method might appear to argue against employing it to generate acceptance rules or to criticise Popper's acceptance rule. Several points can be made in response to this objection:

(1) The failure of the Bayes method when logical probabilities and utilities based on logical probabilities are used does not imply that this method will fail when some other probability measure that does permit probabilities greater than 0 for universal hypotheses is used.

(2) Popper's acceptance rule stands in need of justification regardless of the merits of the Bayes method as a tool for criticism.

(3) Given two universal hypotheses x and z both of which entail evidence y, $C(x, y) = C(z, y)$. According to Popper's rule, no decision can be made between x and y. Yet Popper wishes to maintain that in some circumstances we might feel a decision to be warranted.[1] In order to handle this situation, he introduces comparative measures of content which exhibit 'fine structure' differences in content when measuring content of a hypothesis by means of the prior probability of its negation indicates no such difference. Once Popper is permitted to use such procedures, fine structure measures of $U(x, y)$ should also be permissible. Thus, if $U(x, y)$ is greater than $U(z, y)$ for sufficiently large universes, we might treat $U(x, y)$ as greater in a 'fine structure' sense than $U(z, y)$ for infinite universes even though in such universes $U(x, y) = U(z, y)$.

The upshot is that given the lengths to which Popper is prepared to go to defend his notion of corroboration $U(x, y)$ still has more to recommend it. It takes into account Popper's expressly stated views concerning the scientific importance of hypotheses while weighting such importance with probability in a function the maximisation of which is justifiable according to current theories of rational preference. As an extra bonus, $U(x, y)$ satisfies Popper's desiderata to a good approximation save when $P(x) = 0$. Finally, if in this exceptional but important case, we are permitted to use fine structure measures analogous to those Popper uses anyway, we get a qualitative analogue to $U(x\ y)$, maximisation of which could be supported by a qualitative analogue of the Bayesian approach.

I. LEVI

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THE SUPERCOMPUTER AS LIAR

I HAVE suggested that the best way to show that robots can feel is to build one that (*a*) acts as if it does, (*b*) tells the truth, and (*c*) says it does.[2] The apparent difficulty is with condition (*b*), and two commentators have argued for its indecisiveness or unattainability.[3]

1 Dr Danto considers sympathetically the view that ordinary usage prohibits the application of the predicates 'think', 'feels', 'wants', 'understands questions', to

[1] Ibid., pp. 373-7, especially footnote 13, p. 374 and the paragraphs immediately following the paragraph to which that footnote is appended.

[2] 'The Compleat Robot' in *Dimensions of Mind* (ed.) S. Hook, N.Y.U. Press, 1960

[3] A. C. Danto, 'On Consciousness in Machines,' ibid., and Daya Krishna, '"Lying" and the Compleat Robot,' in this *Journal*. 1961, 12, 146–9

machines. While it is true that applying these predicates to existing machines would be incorrect, I think our grounds for the refusal are their performatory limitations, and not a linguistic prohibition. Dr Danto uncovers many serious difficulties with the ordinary usage argument here, although he does not draw any final conclusion about the validity of my proposed test, on which, of course, hinges the decision whether machines that pass it, which he calls Scriven Machines, are conscious. (Hence I am unfortunately not able to decide whether he gives them this title believing they cannot lie, or believing they cannot tell the truth either.)

2 Dr Danto thinks that even if Scriven machines were conscious, their construction would not affect the major positions on the mind-body problem. I think it would make strong two-substance theories, e.g., soul theories, extremely implausible though not wholly untenable, in a way rather like the way in which evolutionary theory undercut the doctrine of the divine creation of man.

3 Mr Krishna makes a number of interesting points about the concept of lying, with one of which I agree (that lies are contrary to one's beliefs rather than one's knowledge). In bringing these to bear on my argument, however, he makes a step which I found unconvincing, and which certainly begs the entire question. 'One does not introduce robots to "concepts" nor does one "explain" anything to them. One just builds into them structures which perform the behavioural responses . . .' (p. 147). It is not clear that these alternatives are exclusive: my paper is an attempt to show they are not, i.e. that we can *make* machines that understand.

4 He assumes that 'lying' means 'lying successfully' in his main criticism (p. 148). But we talk of 'transparent lies' without contradiction. The same kind of error is involved in 'Professor Scriven has forgotten that if the poor robot cannot but tell the truth, it is a robot indeed.' He presumably means by this that it is not conscious. But a compulsively truthful person, like a compulsive smoker or liar, is not thereby deprived of humanity: his next sentence is surely wrong. '"Truth-telling", like "lying" cannot be mechanised, that is, made compulsory without losing all that we connote by these terms in their human context' (p. 148).

5 Mr Krishna invalidly argues that it is difficult to tell the truth because 'the question what is truth has been the subject of unending debate. . . .' It is not hard to count although it is hard to define number.

6 Mr Krishna argues that a compulsively truthful robot would be predictable in a non-human way and thus fail on another criterion of human behaviour which I discussed. The error of point 4 is again involved, and in addition the assumption that compulsive truth-telling in the robot is incurable. We can cut out the relevant circuit, once its use has enabled us to answer the crucial question for which it was designed, and thereby make the robot capable of lying when it so chooses.

7 In conclusion, I think the strength of the proposed strategy rests on the fact that it duplicates what I believe to be the most important procedure for establishing the existence of feelings in other humans. This is not simply argument from analogy, but from *analogy* (which established the *possibility*) plus *reliability*—in the testimony of certain other humans, often about testable matters—which establishes the *virtual certainty*.[1] Against the use of this argument for robots, Dr Watanabe of IBM maintains that it is 'obvious according to well-disciplined common sense' that only

[1] I think the point of the 'reversed spectrum' example supports the crucial assumption that the robot can understand fully the question it is being asked.

protein based entities can be conscious'.[1] This strikes me as not only empirically false, but also rather subversive: IBM plants and offices are always heavily decorated with little signs, whose presence in the computer rooms I had always felt to show a friendly, forward-looking (or at least faith-healing) spirit—I mean the ones that say 'Think'!

MICHAEL SCRIVEN

SCHLICK'S TREATMENT OF DETERMINISM

IN his paper 'Causality in Contemporary Physics'[2] M. Schlick takes the view that the problem of determinism arises from a metaphysical connotation, attributed to the word 'Determined'. In my paper 'Condition, Cause, Free Will and the Direction of Time',[3] I claim that the problem of determinism is a real one which has not been solved. In my view the entire formation of concepts, linguistic as well as scientific, is pervaded by a certain dualism: concepts that make sense in a determined as well as in an undetermined world, and others that presuppose free will and an undetermined future. In this note, I shall discuss only three of Schlick's arguments, which can be considered without reference to his earlier writings.

(1) According to Schlick's definition, an event is determined if it can be foretold by reason of known laws of nature, undetermined if it cannot be so foretold. Whether a specific event is determined would therefore depend on human thinking and on the state of knowledge at a given time. Schlick concedes that with our present knowledge we cannot calculate all events in advance, and that we are faced with an infinite task. If we adopt his definition of 'determined', we can express the contention of the determinist as follows: 'As each integer can be reached by a finite number of steps, any specific event can be foretold (with an accuracy previously postulated) with the aid of only a finite number of laws.' This is an extrapolation from existing knowledge. We can accept or reject it. As it is an extrapolation *ad infinitum* it can never be proved. However the indeterminist who believes in free will takes the view that it is disproved by every deliberate movement of any part of our body, and that every purposeful act presupposes that the course of events can be changed, i.e. that the course could not have been predicted. As Schlick does not agree with this view of the indeterminist, I cannot understand how he fails to see that here is a real problem.

(2) Schlick states that an event cannot have the characteristic of being determined or undetermined. This is certainly true for any event as it is described in generalised form by concepts and language. For this description takes the individual event out of the actual development of which it forms part, without even fully describing it. And it is this development which may be determined or undetermined. In my paper, I have used the metaphor of a chain for such a course of events. To form part of a chain that starts freely at one end is not a characteristic of the individual links. If anyone argued that it is a characteristic of the first link, the indeterminist would answer: 'The first link of the chain is the act of free will, a personal experience which—as "blue" and "now"—cannot be explained any further. Without this act, the course of events would not have been the same.'

[1] 'The Dimension of Mind', op. cit., p. 146
[2] This *Journal*, 1961-62, **12**, 177, 281
[3] This *Journal*, 1960, **11**, 212

(3) The Aristotelian 'paradox of logical determinism' is not disposed of by Schlick's definition of the concept 'being determined'. This problem can be derived from the concept of choice by free will. Example: Yesterday I had the choice between killing a certain man and not killing him. I decided to kill him and did so. The proposition 'The killing of this man was my deed' is true today. According to Schlick, the truth of a proposition is a timeless characteristic. Then the proposition 'The killing of this man will be my deed' was true the day before yesterday. No act of free will however can change a timeless truth. Accordingly, it was impossible for me not to kill him, and I did not have a choice. The same considerations apply if I did not kill him. Therefore Aristotle's paradox is not solved.

What Schlick considers metaphysical in the popular conception of causality, is what I in my paper have called 'dynamic causality'. It is contained in the physicist's idea of an impressed force (Newton's 'vis impressa'). It is derived from the experience that we can change the course of events by acts based on free will. It cannot be considered metaphysical unless we believe that free will is an illusion.

In this note, I have used the concept 'freedom' in the sense in which it is used by an indeterminist. In his book *Problems of Ethics*,[1] Schlick gives a different meaning to this concept. He upholds that freedom of action can only mean that our thoughts and actions can take their predetermined and foreseeable course without external interference, and that the indeterminist use of the concept is based on a confusion of 'freedom' with 'chance'. Indeed, if one accepts Schlick's definition of chance as comprising everything that cannot be foretold by laws of nature, 'freedom' in the indeterminist's sense forms part of this concept. But this does not prove that freedom of action is an illusion. I shall however not deal with this problem any further, as I want to confine myself to a discussion of Schlick's paper in this journal.

H. C. PLAUT

DISCUSSION OF PROFESSOR PANETH'S ARTICLE

IT was refreshing and instructive to read Professor Paneth's article on the chemical concept of element in the May and August issues of this *Journal*.

On a small point of the history of the philosophy of science, I thought Professor Paneth's reference to Locke and the doctrine of primary and secondary qualities needs some amplification. It is true that this doctrine was a revival of 'the old view, taken by the Greek atomists, that only size, shape and motion are real. . . .' Also it is true that this doctrine was revived by Locke and expounded in the celebrated *Essay*.

I suggest, however, that it is undesirable to leave out the names of Galileo and Descartes in this matter, because not only did they revive the doctrine before Locke; the doctrine itself took somewhat different forms, when expounded by different thinkers.

As early as 1623 Galileo wrote these words in *Il Saggiatore:* 'But I do not believe that there exists anything in external bodies for exciting tastes, smells and sounds but size, shape, quantity, and motion, swift or slow; and if ears, tongues, and noses

[1] English Translation, New York, 1939

were removed, I am of opinion that shape, quantity, and motion would remain, but there would be an end of smells, tastes, and sounds. . . .' It is almost true, as Whitehead said, that on this subject Galileo 'leaves nothing for Descartes and Locke to do. . . .'

Descartes wrote extensively on the doctrine of qualities. A particularly good and clear account of the Cartesian doctrine is to be found in the Second Part of the *Principia Philosophiae* (1644). It has two very interesting negative characteristics. It is not associated with any kind of atomic speculation, and it does not include amongst the primary qualities either the 'quantity' of Galileo or the 'solidity' of Locke. What is *really there*, outside our minds, is purely geometrical and kinematic, as if the common objects of everyday life are all allotropic modifications of extension.

Locke did something other and more than revive the ancient doctrine of Democritus; he modified significantly the Cartesian philosophy which was fashionable in his day. In Book II, Chapter VIII of the *Essay* (1690) Locke lists the primary qualities as 'solidity, extension, figure, motion or rest, and number'.

At the beginning of Book III of the *Principia* (1687) Newton gives what I think it would be reasonable to call his own definition of primary qualities. He describes them as 'the universal qualities of all bodies whatsoever'. He goes on to show that the 'force of gravity' is not such a quality, in this agreeing with Descartes, who describes 'weight' as a secondary quality; but that 'mass' is such a quality for, in his own words, mass 'is immutable'. By his discovery of mass, Newton was able to put a much straighter edge, so to say, on the doctrine of qualities as expounded by Locke.

Locke (*Essay*, Book II, Chapter XXIII) and Newton (*Opticks*, Book III, Part I) revived the philosophy of Democritus in its original atomic form. This in fact gives to Newton's account of the doctrine of qualities a somewhat antiquated air. Descartes's extended and classical statement of the doctrine reminds us that atomism is not necessarily a concomitant of the doctrine. Atomism in general, and the 'two kinds of quality' doctrine in general, are both types of 'a reality behind the appearances' philosophy of nature. Often, for example in either Democritus or Eddington, the two types of philosophy are associated together. But such an association is not necessary.

JOHN BRADLEY

F. A. Paneth's Works and Translations

Sir,

In connection with 'The Epistemological Status of the Chemical Concept of Element' by F. A. Paneth, published in this *Journal* in May and August 1962, it may be of interest that this article partly overlaps with one by the same author on Mendeleeff to be published in translation by Interscience under the editorship of Professor H. Dingle and Dr G. Martin; and that the main technical terms in the two translations are rendered in both cases by the following:

Materie	matter
Urbestandteile	primary constituents
Urstoff	primary substance

Stoff	substance
Grundstoff	basic substance
Einfacher Stoff	simple substance

There are, however, two instances in which it appears to have been found desirable to render 'Stoff' by 'matter' (this *Journal*, 1962, **13**, 5, 9); though elsewhere it is rendered 'substance'.

Further, the original of the article in this *Journal* will be available in a selection of Paneth's works, *Auswahl aus den Schriften*, shortly to be published by Vieweg, Braunschweig.

Yours faithfully,
EVA PANETH

REVIEWS

Erkenntnislehre. By Viktor Kraft.
Springer-Verlag, Vienna, 1960. Pp. v + 375.

IN his latest book Professor Kraft attempts ' to give a systematic presentation of the theory of knowledge in its present state of development ' (iii). One of the main reasons for this enterprise lies in the fact that the new developments in philosophy which started with the Vienna Circle ' have not been taken into account by the German books on the " theory " of knowledge ' (iii). American books, on the other hand ' use great precision in deriving theorems from presuppositions which postulate what would seem to be in need of investigation. . . . In this way the problems are put aside, they are not dealt with. But it is the task of epistemology to proceed to the foundations and to clarify them ' (iv).

Professor Kraft starts accordingly with an investigation of the nature of knowledge and of the manner in which the essential characteristics of knowledge can be determined. This investigation is a masterpiece of clarity and incisive argument and it should be read by anyone intending to take up a problem of epistemology or of the philosophy of science. The main thesis of this chapter is that ' the idea of knowledge can be arrived at only on the basis of stipulations and its validity is a matter of agreement (with these stipulations) ' (28). The theory of knowledge is therefore ' very different from the factual sciences; it does not deal with something existing in reality but puts forth aims and norms for our intellectual activity ' (32). It suggests an *ideal* which we use to criticise or to praise theories, statements, points of view which are announced together with the claim that they represent knowledge (23). It ' is supposed to be the basis for a criticism of actual theorizing and not an activity which merely describes such theorizing ' (24).

This is radically different from both the traditional views (of which Platonism, phenomenology, transcendental idealism, and inductive metaphysics are discussed) and from what is implicit in many philosophical investigations of today. In its early days logical positivism was bold enough to oppose even the sciences and to demand that they be changed radically and freed from their metaphysical ingredients. This was an *unreasonable* ideal, no doubt, but being an *ideal* it provided a motor for change and for critical examination of the existing theories. It also led to the construction of new theories (the elementary quantum theory is an example). And it is quite clear that modern science itself could not have come into being without such a motor. At its beginning it was very different indeed, both as regards its content and as regards the methods used for its augmentation, from the

existing body of doctrine—Aristotelian physics. Those who built it compared the existing things with an ideal and found them wanting. It is for this reason that they were capable of changing the whole outlook of their age, the attitude towards religion included. Such radical optimism is hardly found today. Especially within philosophy proper the realisation of the limitations of the early doctrines of the Vienna Circle as well as the general commotion created by the progress of the sciences have led to a most submissive attitude towards the actual form of the sciences and later even towards the opinions inherent in the natural languages. Such an attitude is today also exhibited towards subjects which are still in their infantile stage. They are not criticised on the basis of a perhaps somewhat strict ideal and thereby given a chance to improve, they are rather said to possess a ' logic of their own ' according to which they must be judged; which means, of course, that they are not judged at all. An analysis like the one carried out by Professor Kraft in the first chapter of this book should convince everyone that such a procedure is neither possible, nor desirable. It is not possible as any investigation into the actual form of some enterprise proceeds by selection (clearly not *every* move of a scientist can and should be recorded and described by the philosophy of science) and, thereby, by implicit reference to an ideal (9). And it is not desirable as we should be clear about our preferences rather than introduce them in an underhand fashion.

It is unfortunate that Professor Kraft does not follow up this excellent description of the *nature* of epistemology by an equally clear development of the *norms* adopted, of the *reasons* for the adoption of these norms, as well as by a detailed argument showing how his position on various issues is influenced by these norms. This would have been a revolutionary undertaking indeed, the first construction of a purely normative epistemology. As it is there are many issues in the book which are discussed in the traditional fashion. However those cases where the reference to norms is made explicit show very well the power of the new mode of thinking. An example (and the only example I shall be able to discuss in the present review) is the way in which Professor Kraft treats the problem of induction, the problem of the existence of material objects, and the problem of the existence of other minds.

Professor Kraft argues very convincingly that the problem of induction is still without a solution. He discusses with excellent reasons the inadequacies of the probabilistic theories, notably of the theories of Reichenbach and Carnap (the main objections against the latter are inapplicability, to problems of physics, of the formal systems used (233 f.) as well as the fact that in a world with infinitely many individuals any general statement has probability zero on the basis of finite evidence (232); instance-confirmation for which a finite value is obtained is rejected with the remark that the sciences cannot do without laws which are needed for the purpose of

explanation.) The conclusion of the discussion is that the step beyond experimental results can be made only with the help of *hypotheses* for which little or no initial justification is given. A point of view that is used for providing knowledge about reality is a system of such hypotheses (243). And ' what is regarded as induction is nothing but the process of introducing hypotheses on the basis of (the known) facts. . . . Induction is therefore not a new and specific method of justification. There does not exist any valid method of justification besides deduction ' (253).

In the Anglo-American literature this doctrine has become associated with the name of Professor Karl Popper of the London School of Economics. It is therefore not unimportant to point out that Kraft has developed his own ideas independently, and earlier [in his *Grundformen der Wissenschaftlichen Methoden* which was published in the year 1925 (ten years before the publication of the German edition of the *Logic of Scientific Discovery*) in Vol. 203 of the *Sitzungsberichte der Oesterreichischen Akademie der Wissenschaften*; this essay deals both with the natural sciences and the social sciences; it still deserves careful study by anyone interested in the philosophy of these subjects. Popper himself refers to Kraft as one of his predecessors.] There are also differences between Kraft's point of view and that of Popper. An example is his attitude towards the problem of observation. According to Popper test-statements (' basic-statements ') are about observable relations between macroscopic objects. They are *motivated* by experience, but not *justified* by it. Their interpretation is ' materialistic '. According to Kraft, however, the ' hypothesis of a world of objects which exists outside the reality of our experiences can only be tested on the basis of experience; . . . the reason is that all other facts already involve the hypothesis to be tested ' (294).

Now the important thing is that for Kraft test-statements are not about single perceptions, or sensations—this is the position which has been so effectively criticised by Popper—but about ' perceptions, insofar as they are members of a context regulated by laws ' (293), or about *bundles of perceptions*.

It is worthwhile to look a little more into the theory behind this interpretation of the test statements. Two domains are assumed, the domain of really existing objects, and the domain of perceptions. Perceptions are not sensations; ' without the knowledge added to the sense-data we would not possess any perception of material objects ' (285). Nor are perceptions parts of real objects: ' what is given in experience is not the object . . . itself. . . . This object is essentially unperceivable ' (291). What we meet in the world of experiences are *signs* for the objects (*Anzeichen*). But—and now comes the important step—these signs are no longer identified with either sensations, or with *single* perceptions. What represents a material object (and, for that matter, what represents somebody else's mind) in the domain of experience is a *bundle* of perceptions lawfully connected and it is to this bundle that

observation sentences refer. The charge of psychologism is thereby successfully avoided.

However it might now be objected that the physical objects postulated in addition to their representatives on the plane of observation are completely superfluous. The theory has great similarity with Kant's point of view. The domain of experience corresponds to Kant's domain of phenomena or to his 'nature' which is just phenomena connected by laws. And the real objects seem to be very similar to the *Ding an sich* of Kant: they are not accessible to direct observation, they are postulated in addition to the perceptions and they seem to be altogether useless. This objection overlooks the very important difference between Kraft's theory and the theory of Kant which may be summarised in the following two important points: first, Kraft establishes a connection between the postulated real objects and experience which allows him to control properties of the former by reference to the latter; the second difference lies in the fact that methodological, normative considerations are given for the acceptance of the hypothesis of real things and of other minds. This must now be explained in some detail.

As in the case of the problem of induction Kraft points out that almost all procedures of the sciences and of common sense (observation and measurement included) involve hypotheses such as the hypothesis of the existence of material things and the hypothesis of the existence of other minds. He makes it very clear that these two ideas are *hypotheses*, that they are unjustified conjectures for which no foundation can be given. This is done, partly, by a detailed examination and refutation of traditional and more 'modern' arguments attempting to *prove* them in one way or another. He also argues very convincingly that the only alternative to the use of these hypotheses is *solipsism*. In this he is much more straightforward than many philosophers have been who wanted to have their cake and eat it, i.e. who wated to drop 'metaphysical' ideas such as the idea of the reality of the external world, and who hoped that this could be done without ending up in absurdity. I think Professor Kraft has exhibited their methods very well. We therefore seem to be left with a rather uncomfortable choice—solipsism, or a body of doctrine (or opinion) which uses unjustified conjectures as essential parts. Is there a way out of this dilemma?

There is, and the solution is purely methodological, or normative. It is also very simple and contains two demands only. First, that our ideas be *testable*. This demand invites us to connect out hypotheses with the domain of experience. Second, that our ideas be capable of giving an explanation of known phenomena. This demand specifies the connection with experience in detail and also puts restrictions upon the ideas to be used: the ideas must not be *ad hoc*, they must be richer in content than what they want to explain, that is they must establish connections in the domain of experience which are

either more strict, or more complex, than the connections already known. It also results in their transcending experience—for if the entities postulated for explanation completely coincide with laws in the domain of experience, then they are *ad hoc* with respect to these laws and therefore no longer capable of giving a satisfactory explanation. The hypothesis of the existence of material objects is therefore not only an essential part of our thinking, in the sciences and within common sense, a part without which much apparently valuable knowledge would simply collapse—this, too, is shown in great detail by Professor Kraft—it is also capable of support. The support is not by *proof*, nor by 'induction' it is by methodological argumentation that is by reference to some of the norms which constitute our epistemology. This establishes a decided difference between Kraft's theory of two worlds, and Kant's.

I have dwelt at length on this particular item in order to show how a normative epistemology can take care of traditional problems for which no satisfactory solution is as yet available. I shall therefore have to omit a more detailed discussion of the remaining parts of the book which deal with language, logic, the idea of truth, physical laws, reality, microphysics, and the theory of probability. And in any case it would have been quite impossible to give a half-way satisfactory account of a book which is so rich in content and at the same time written in such a terse style. Let me mention only one further point. Many contemporary philosophers are so convinced of the revolutionary character of their enterprise that they believe they can safely neglect everything that happened before, say, 1900 (or perhaps 1889, the birth-date of Wittgenstein). Wittgenstein himself has brought a completely unhistorical atmosphere into philosophy. Although dealing with problems which have a firm foundation in the history of philosophy he has presented these problems in a fashion that suggested that they had just been born and were completely unrelated to whatever else had been thought before (except perhaps the *Tractatus* and Russell's *Principia*). Kraft's book shows the advantage gained from an acquaintance with the philosophical tradition. It is to be recommended to any serious student of epistemology and of that recent bastard-child of epistemology, the philosophy of science.

P. K. FEYERABEND

Toward Reunion in Philosophy. By Morton White.
University Press, London: Cumberledge, Harvard, 1956. Pp. xv + 308.

THE title of Professor White's study gives expression to his concern about the growth of 'compartmentalisation' of philosophy into special subjects, and indicates an attempt at re-establishing communication between the various compartments. The principal aim of his enquiry is to show that

more insight can be gained by treating the problems of metaphysics, epistemology, and ethics together than by approaching them separately. In his view philosophy is a unified discipline and ought to remain so.

In order to discredit the trichotomy of logical principles, scientific principles, and moral principles the author criticises a number of traditional distinctions which philosophers have come to take for granted. In Part I, which is devoted to the problem of existence, he argues that existential statements of philosophy are not different in kind from existential statements to be found in science or in ordinary usage. In particular there is no reason to regard them as analytic or dismiss them as meaningless. Two positions have been taken in speculations about the word ' exists '. According to the one, which the author calls ' univocalism ', the word ' exists ' is used in one sense only. According to the other, described as ' multivocalism ', the word ' exists ' has as many distinct uses as there are categories. Professor White declares himself in favour of univocalism, and seeks support for his view in the logic of quantifiers as interpreted by Quine. Like Quine he believes that ' inflationary introduction of queer entities is best abandoned in philosophy as the early moderns abandoned occult qualities in physics '. Consequently, he refuses to accept platonism, which he construes as the doctrine asserting the existence of meanings and attributes. He is critical of platonism because in his opinion it has failed to illuminate the notion of understanding or knowing *a priori*.

The distinction between the analytic and the synthetic is examined in Part II in connection with the problem of *a priori* knowledge. In the first place the author criticises Russell's early view that *a priori* statements deal exclusively with universals conceived as attributes. In harmony with Part I he argues against the belief that there must be a domain of special entities, other than particulars, entities which have to be examined by the philosopher before he can know that a statement is true *a priori*. The alternative thesis that all and only *a priori* statements are analytic is found to be defective because the notion of analyticity, which is the key notion in the definition, turns out to be hopelessly obscure. Equally obscure, according to the author, is the notion of synonymy, which has been used by philosophers in their effort to clarify the notion of analyticity. The difficulty is that there seems to be no inherent quality of analyticity to be discovered in a statement before it can be correctly described as analytic. Consider, for instance, the statements ' all and only men are rational animals ' and ' all and only men are featherless bipeds '. Why should the former be called analytic rather than the latter? If we are told that the statement ' all and only men are rational animals ' is analytic because it can be related to a definition, we continue our criticism by asking why the statement ' all and only men are featherless bipeds ' cannot be so related. Conventionalism, which consists in constructing semantical systems in which analyticity of certain

statements is accepted axiomatically, does not throw any light on the way in which the notion of analyticity should be applied in ordinary usage. Moreover, in a semantical system rules to the effect that such and such terms are undefined, such and such expressions are meaningful, such and such statements are axioms or definitions, and such and such statements are analytic, appear to be performatory, and this, the author points out, raises the problem of justifying ' analytic '-making, ' definition '-making, etc.

It is the need to defend this kind of logical legislation that directs the author's attention to ethics, which is the principal topic of Part III. Here again he criticises and rejects both Moore's distinction between natural and non-natural attributes, and the positivistic theory of descriptive and emotive meaning. According to him normative statements are not different in kind from descriptive statements, and in accepting or affirming or believing the former we do precisely what those who accept or affirm or believe the latter do. It is significant, the author points out, that we use phrases ' it is true ' and ' the fact is that ' not only in connection with scientific statements but also in such expressions as ' it is true that this ought to be done ' and ' the fact is that this ought to be done '. Moreover, he argues that there is no reason to believe that there is a special kind of peculiarly ethical argument, neither deductive nor inductive, which carries us from an informative descriptive statement to a singular ethical conclusion, provided the conclusion is an evaluative statement, say, a statement of the form ' it is right to do this or that ' and not a performatory utterance such as ' I approve of this or that ' or ' I like this or that '. Of course, in inferences from a singular descriptive statement to a singular descriptive statement we presuppose a scientific principle whereas in inferences from a singular descriptive statement to a singular evaluative statement an ethical principle is presupposed; but the logic involved in both inferences is the same.

In Part IV, which is the last part of the book, the author sums up the results of the preceding enquiry, and offers an outline of his own philosophical position. He announces a breakdown of the epistemological differences between logic, science, and ethics, and suggests that the principles of these three disciplines form a unified framework of beliefs which collectively help us to organise our sensory experience as well as our moral feelings of approval or revulsion toward actions. With the aid of this framework we predict what will be and what should be. We all know only too well that sometimes our predictions fail to come true and that on occasions arguments involving the use of moral principles lead to conclusions which we find morally unacceptable. The methods of solving such crises are of particular interest to the philosopher. As a rule it has been accepted that falsified predictions call for a revision of scientific theories involved. This view has recently been generalised to the effect that the revision should in principle take into account the logic and mathematics made use of in our

predictive reasonings. Professor White's generalisation goes even further. He points out that moral feelings offended by the conclusion of an argument may constitute a challenge to our whole framework of logical, scientific, and moral principles. In his view we can learn this from the examination of the logic of ethical reasoning, and finally do away with the dualism between logic-cum-empirical science and ethics.

This is a stimulating and instructive book. It will make a very useful companion to the study of such topics as the platonism of the early Russell and Moore, the positivistic approach to meaningfulness and analyticity, and James's pragmatic theory of truth. The principal merits of Professor White's book seem to lie in his penetrating analysis and vigorous criticism of a number of ideas and doctrines which have had considerable impact on the development of philosophical thinking in the last fifty years. The notion of analyticity has perhaps been given more severe treatment in the book than any other notion. It has been denied any objective connotation and in the end the 'analytic'-making of statements has been declared to be utterly useless. The various abstract entities of the philosopher do not fare any better. Their existence is rejected by the author on the ground that they are more mysterious than the epistemological facts which they are supposed to explain. There is, however, one thing that is not at all clear to the reviewer. How does the author reconcile his rejection of 'queer entities' in philosophy with his unqualified acceptance of the logic of Quine, who, as is well known, finds himself compelled to admit the existence of classes? There seems to be some evidence in the book that the author leaves room for some existential statements which cannot be derived from sensory reports. The acceptance of such statements involves, according to him, practical decisions, but he is reluctant to follow Quine in basing these decisions on pragmatic justification. He refers in this context to what Nelson Goodman has called philosophic conscience. The significance of this reference appears to the reviewer somewhat puzzling because with Goodman the talk of philosophical conscience is a figurative way of disclaiming any idea of justification. The attempt to bring descriptive statements and evaluative statements under the heading of informative statements is interesting and may prove successful but a further enquiry into the syntax of evaluative statements will certainly be needed. On the positive side the author offers little more than a programme, which of course will have to prove its feasibility by guiding philosophers to new and more convincing solutions of the problems bequeathed to them by their predecessors. Professor White seems to be aware of this. For he quotes from Locke's 'Epistle to the Reader', and regards his own book as an attempt at removing some of 'the rubbish that lies in the way of knowledge'.

CZESŁAW LEJEWSKI

Social Anthropology. By D. F. Pocock. Newman History and Philosophy of Science Series, No. 7.
London, Sheed and Ward, 1961. Pp. vi + 118. 5s.

MR POCOCK's brief book on 'the problems and methods of social anthropology' is divided into three parts. Parts I and II are his version of the official history of social anthropology up to Radcliffe-Brown; Part III is a discussion of some subsequent problems and developments.

Pocock characterises social anthropology as the science resulting from the interaction of sociological *speculation* about the role of man in society—taking in, often enough, moral problems—and anthropological *description* of man and his behaviour. Before recounting its history he argues that 'society could not be studied in anything resembling a scientific manner until the idea of society as in some sense an object to be studied had been established' (p. 5). Until things social (or conventional) were differentiated from things natural, ideas like Rousseau's that free-born man is enslaved by 'Society' were not possible. A further development, social life, being *sui generis*, requires a science of its own which cannot be 'reduced', came only with Montesquieu. It was reaffirmed by Comte who, according to Pocock, argued that

> If society exists . . . it must be studied positively as it exists, and the analysis of collective phenomena must preserve the integrity of their unity and not seek for explanations in terms of an introspective psychology (p. 18).

Subsequently, Pocock relates, Herbert Spencer extended Darwin's theory of evolution to the study of society with the result that evolutionary schemes became the standard framework into which anthropological facts were fitted. Moral judgments—the idea of a progress from primitive to civilised—played a big role in evolutionism. Early theories held that social evolution was unilinear, but were abandoned under diffusionist criticism; even with revised evolutionism, however, it was apparent 'how inconclusive the argument must remain while scholars had to rely upon the fragmentary reports of others' (p. 26). This situation leads to a further consideration. The main problem presented by these reports was that they isolated individual customs from the social context which gave them meaning. The solution lay in systematic reports which did justice to the contextual meaning of customs.

Pocock points out that the reporting of facts about remote or primitive peoples is as old as travellers and books. 'Vico, Montesquieu, Hume, Ferguson, Rousseau . . . incorporated' (p. 16) such knowledge into their theories. But to report the facts of a society as constituting a meaningful system required a theory of such systems. It could not come from England because the empiricist tradition disavowed preconceived theories; i.e. 'each observer had his own interests and convictions . . . [and] could not prevent

his surmises . . . from imposing their categories upon his description ' (p. 19). It was from France that the framework of theory into which to fit (and with which systematically to observe) the facts came. The influence of the French School did not cross the channel, however, until after the First World War.

It was at about the same time, according to Pocock, that fieldwork began in earnest. Malinowski's internment on the Trobriand Islands during the First World War gave him an unrivalled opportunity to get to know a primitive society really thoroughly. He then came to England and began to train pupils. Their work created the need for a theory ' to order the new mass of facts which were [*sic*] being collected ' (p. 54). The need was met by Durkheim's theory, presented, and significantly modified by, Radcliffe-Brown. He argued that society was an adaptive mechanism facilitating an ordered social life in a given environment; it was a well-integrated mechanism; and the aim of social science was to seek out the atemporal structural patterns of the mechanism, so that by making comparison between these patterns, generalisations could be built. Despite the circular arguments of the theory of integration; despite the lack of interest in social change consequent on the atemporality; despite the neglect of the individual for the structure; Radcliffe-Brown's theory was accepted, while the less restricted theories of Parsons, for example, were not. Pocock explains that this was because Radcliffe-Brown's theory was the only available one which did not go beyond the facts and which was useful to the fieldworker trying to analyse small, preliterate societies.

Part III is about the objectivity of social anthropology and what has happened since Radcliffe-Brown. How can an observer, influenced by his own social background, be objective about another society? Pocock suggests that the anthropologist's objectivity is like that of a translator who renders one tongue into another; in his sociological language lies his objectivity. But since language is part of a meaningful social context any discussion or description in another language involves an inevitable departure from the experience which that society has . . . of itself ' (p. 104). If instead social scientists always used the indigenous language ' there could in fact be no sociology ' (p. 105). So he holds that a sociological language is a prerequisite of sociology and the richer it is the greater the objectivity conferred.

Pocock argues that the principal advances since Radcliffe-Brown are: (i) a re-emphasis on the individual rather than the structure: (ii) a real attempt—attributed to Evans-Pritchard—to interpret society as a meaningful system *with a time-dimension* rather than as an atemporal well-integrated structure. The comparative method is then reinterpreted: comparison, Pocock maintains, is involved in all thinking about society; but such implicit comparison deepens understanding, it does not lead to generalisations.

This very reasonably priced volume is to be recommended. The official history is clearly presented. Part III, however, after the criticisms of Radcliffe-Brown have been made, is not so clear. In particular, I found the discussions of objectivity and of meaning hard to follow. It is admirable that Pocock wants an objective social science which avoids both social determinism and moral relativism, but I doubt whether the idea of objectivity he develops is strong enough to do this. For a language of sociology (like any language of science), while sometimes convenient, strikes me as only one of the institutions making for objectivity, and not a necessary one at that.

Finally, meaning: when I read Evans-Pritchard's own presentation of his divergences from structuralist orthodoxy I thought I had understood what he was getting at, namely the need to supplement structuralism with historical understanding. Pocock, and some of Evans-Pritchard's other pupils, are given to saying that Evans-Pritchard shifts the emphasis from structure to meaning,[1] and they seem to believe that these two accounts of his unorthodoxies amount to much the same. To me they are not the same so if Pocock's formulation is correct the obvious reading of Evans-Pritchard is an unsuspected misunderstanding which might with advantage have been brought out and cleared up.

I. C. JARVIE

Models and Analogues in Biology. Symposia of the Society for Experimental Biology. No. 14.
Cambridge University Press, 1960. Pp. 255. 50s.

THE organiser of this Symposium, J. L. Beament, remarks in the course of his own paper on 'Physical Models in Biology,' 'Since eighteen contributors to this Symposium have that number of definitions of the terms "model" and "analogue", another can hardly be out of place.' That makes nineteen. For the philosophical reader, therefore, the Symposium offers a somewhat bewildering set of data bearing on the questions: 'What is a

[1] This curious introduction of the word 'meaning' has puzzled me for some time. I now have a conjecture as to how it arose. The basic fallacy of structuralism is that, where *a* implies *b*, it claims to explain antecedent by consequent, *a* by *b*. It is argued, for example, that incest taboos lead to social stability ($a \supset b$); and thence that social stability, or the society's need for it, explains incest taboos (*b* explains *a*). Now this is an obvious logical mistake. But there is something worth preserving in the theory; the incest taboo does have this stabilising effect. So it is instead suggested that social stability does not of course *explain* the incest taboo, but it does give us the sociological importance or *meaning* of the incest taboo. The introduction of 'meaning' is a surreptitious attempt to evade the fallaciousness of structuralist 'explanations'. May be Evans-Pricthard was saying this, but I doubt it. I thought he said that *b* is explained by *a* (or partially so) and that an explanation of *a* must needs be mostly historical, therefore *b* will in the end be explained historically too. The pupils seem to have appreciated the difficulties of structuralism less fully than Evans-Pritchard himself.

model?' and 'How do models function in scientific explanation?' Some light—though not enough for a volume of this title—is also shed on the question whether the nature and use of models in biology differs in any significant way from their character and use in the physical sciences. As to the first pair of questions: if the reader believes, naïvely, that 'models' are *things* like the mechanical models R. H. J. Brown is talking about in his paper ('Mechanical Models in Zoology') or like the Grey Walter beetles and other hardware automata mentioned by F. H. George (on 'Models in Cybernetics'), H. Kacser (in his paper, 'Kinetic Models of Development and Heredity') will put him firmly in his place. This, he declares, is the 'colloquial use of the word', characteristic of 'little boys'. 'A model', Kacser pronounces, 'is a statement or a series of statements in language.' A. W. F. Edwards, in his paper on 'Models in Genetics', appears to agree. 'It is clear', he says, 'that the words "hypothesis", "theory", and "law" have the same logical status as model, *in so far as all of them are propositional in nature and in fact used in the same way*' (my italics). On the other hand, Professor W. T. Williams, in his delightful paper on 'The Problem of Communication in Biological Teaching', distinguishes between propositions as 'first-order models' and other models (or analogues) as 'second-order'. Some other contributors, like J. W. S. Pringle, who discusses 'Models of Muscle', allow for the existence of both 'conceptual' and 'actual' models—the latter being the sort that most chemists and some biologists, as well as 'little boys', call by this name. Still others, like J. H. Westcott (whose contribution concerns the 'Estimation of Values of Parameters of a Model to Conform with Observations'), restrict 'model' to conceptual systems and use 'analogue' for actual working constructs.

Now it is of course possible to restrict the use of 'model' to conceptual or mathematical models, and so to 'statements' of some kind. And at the same time it is possible to extend the meaning of 'model' in such a way as to make all 'laws' or 'hypotheses' models. For any theoretical statement is in some sense a map to guide us through experience and yet it is not experience but only a guide to it somehow mirroring its structure, and so a 'model' of it. But at this stage, it seems to me, on the one hand, the question what a model is becomes primarily one of terminology: you can call all hypotheses models if you want to and you can confine the use of the word to conceptual models only, if you insist. By the same procedure, on the other hand, the question what a model can do is begged. There is a traditional division of scientific explanation into explanation by analogy, usually of the less by the more familiar, and mathematical explanation, by the formulation of equations which then may or may not be related to experimental data by the use of models. This division may be mistaken or unnecessary, but it does underline the fact that at least some explanations work by analogy whereas others appear not to, and so it leads us to ask what

the rôle of analogy is in explanation. The question what a model can do is then part of the general problem of the explanatory uses of analogy, a problem which the Kacser-Edwards axis simply bypasses. (Incidentally, Edwards makes the puzzling statement that among models, in his sense, the 'probabilistic model' is the only one which may actually lead us, rather than merely pointing the way, since 'here we may make inferences, in terms of probability statements, about the real world'. Is this true? Perhaps, in so far as the statistician is working, within the bounds of prior and posterior probability, in an ideally self-contained world, and therefore one which, so far as randomisation is not interfered with, must be mirrored in reality. Yet at the same time Edwards asserts that 'the process of randomisation destroys information that would otherwise be available'. The cake so proudly displayed would seem to be already digested.)

As to the third question, of the uses of models specifically in biology, the contribution of most interest philosophically seems to me that of R. A. Hinde on 'Energy Models of Motivation'. Hinde discusses the 'models or theories' of Freud, McDougall, Lorenz, and Tinbergen, and lists some of the dangers inherent in the use of such models. Behavioural energy need not, he argues, share any of the properties of physical energy, and the inference that it does so has often misled or hindered research. For example, displacement activities were explained on the energy model by supposing that the thwarted energy 'sparked over' into the displacement activity—sparking over being a property of (electrical) energy which was imputed to the behavioural energy. This idea hindered an analytical study of the causal factors underlying displacement behaviour. Similarly, the unitary nature of the 'energy' explanation often misleads, since a single 'drive' variable may mask significant diversities in behaviour; or conversely, emphasis on the alleged independence of energies acting in different structures may lead to neglect of factors conjointly affecting many activities. Dr Hinde has discussed some of these problems in other places, including this journal (1956, **6**, 321-31). His analysis opens up two lines of philosophical interest: in general, the rôle of concepts in guiding, or misguiding, empirical research, and in particular, the relation of physical models to biological investigation.

On broader philosophical lines, Niels Bohr's opening paper calls attention to the rôle of 'complementarity' in biology. In an opposite direction, the framework of George's exposition of cybernetical models for the study of vision raises the whole issue of reductivism: cybernetics provides, he believes, 'the thin edge of the wedge which will make biology mathematical'; it is the modern heir of eighteenth-century materialism. And against this reductivist hope, Professor Williams's paper suggests the limits of formal methods in the communication of skills in research and teaching, and hence in the mathematisation of some activities of higher animals. In

general, however, one would expect this Symposium to provide, in a rather haphazard way, technical material of use to a variety of specialists rather than a common conceptual basis for methodological and philosophical problems. It is regrettable that there was no occasion for a meeting of minds on the question of the nature and rôle of models and analogues in biology.

MARJORIE GRENE

The Correspondence of Isaac Newton. Vol. III: 1688–1694. Edited by H. W. Turnbull.
Cambridge University Press (for the Royal Society), 1961. Pp. xviii +445. 7 guineas.

FATE has not dealt kindly with those associated with this enterprise: Professor H. C. Plummer, the first editor, died just after the war; in the preface to this latest volume to be published, his successor records a severe setback to its preparation following the sudden death on 22 April 1960 of H. W. Robinson, the former Librarian of the Royal Society, who was an active member of the Editorial Board; then, on 4 May 1961, Professor Turnbull himself died; and on the following day, his indefatigable and learned correspondent J. E. Bullard, to whom so many of the annotations in the present and previous volumes are due, also died, quite unexpectedly.

The current volume (containing items Nos. 319 to 466) maintains the same high standard of scholarship and book production set by its predecessors and comprises much material of absorbing interest. It opens in December 1688, one of the first documents (No. 324) being a copy of the Certificate of Election of Sir Robert Sawyer and Isaac Newton to Parliament as Representatives for the University of Cambridge. It is interesting to note that in the election to the Convention Parliament there were three candidates, Newton obtaining 122 votes and the others 125 and 117, respectively. Newton's fellow burgess was senior counsel for the Seven Bishops in 1688. There was much mob violence at the time, not least in Cambridge, and in a letter from London to the Vice-Chancellor, accompanying the form of the proclamation of William and Mary, Newton writes that he ' could wish heartily that ye University would so compose themselves as to perform ye solemnity with a seasonable decorum '. In his Preface to the present volume, Professor Turnbull draws attention to the remarkable fact that, even when engaged on the colossal task of writing the *Principia*, Newton had found time ' to perform such public duties as defending the freedom of the universities '.

The publication of the *Principia*, in 1687, gave rise to much correspondence which proved a sore trial to Newton's patience. Pre-eminent among Newton's correspondents in this period were Leibniz, Huygens, Locke, and Bentley. The present volume contains the only two letters known to have

been exchanged *directly* between Leibniz and Newton (Nos. 407 and 427). Dated 7 March 1692/3, Leibniz's letter refers to 'a devotion that has lost nothing by the silence of so many years'. In referring to Newton's 'astonishing discovery' concerning gravitation and Kepler's ellipses, he writes: 'And yet I would be inclined to believe that all these are caused or regulated by the motion of a fluid medium.' In his reply of 16 October 1693, Newton attributes his tardiness to mislaying Leibniz's letter among his papers. He maintains that vortices serve only to 'disturb' the motions of the planets. Since, in his opinion, all phenomena of the heavens and sea follow *precisely* from his laws of motion and of gravitation, and since nature is 'very simple', he argues that all other causes must be rejected and 'the heavens are to be stripped as far as may be of all matter, lest the motions of planets and comets be hindered or rendered irregular'. Newton admits that, if someone should succeed in explaining gravitation and its laws by the action of some 'subtle matter' and shows that this will not in any way disturb the motion of planets and comets, 'I shall be far from objecting'. But it is clear that he does not think that this is likely to happen.

Of outstanding interest in the present volume are three manuscripts (Nos. 347–9) of 1665–6 and 1672, respectively, published out of their chronological order because they throw light on the origins of Newton's thought upon motion and gravitation. Only a part of the first has been published previously. This, the most interesting of the three, is concerned with the evaluation of the gravitational acceleration constant g. It is well known that Galileo's value (implicit in his results) was only about half of what it should be. There is convincing evidence that Newton's work was prompted by a passage in Salusbury's translation of Galileo's *Dialogues*, published in 1661. (The discovery of this link with Galileo must be regarded as a major find.) Newton's own evaluation of g, in this manuscript, depended on experiments with both the simple and the conical pendulum. He obtained five independent determinations ranging from 32·5 to 33·0 ft. per second per second. Although, in 1659, Huygens had already obtained the value which is equivalent to the modern 981 cm. per second per second, Newton's was an outstanding independent achievement, for Huygens did not publish his result (in intelligible form) until 1673.

Included in this volume are a series of letters on textual criticism of the scriptures to John Locke and John Mill, several of which have not previously been published. In the opinion of the Editor, this correspondence 'places Newton in the forefront among biblical scholars of the time'.

Among the many other absorbing topics treated in this volume are the method of fluxions, the nature of acids, and the distribution of matter in the universe (in the celebrated correspondence with Bentley). Also there is a particularly interesting exchange of letters with Pepys. This begins very unpromisingly with Newton (No. 420, 13 September 1693) complaining

that 'I am extremely troubled at the embroilment I am in, and have neither ate nor slept well this twelve month, nor have my former consistency of mind. . . . I must withdraw from your acquaintance, and see neither you nor any of my friends any more, if I may but leave them quietly.' However, he is soon explaining with great care the application of the laws of probability to games of chance. At the suggestion of J. E. Bullard, the particular problem in question was considered further by T. W. Chaundy and a joint paper was published (*Math. Gazette*, **44**, 1960, 253-260). Curiously enough, all concerned seem to have overlooked an interesting historical note on this little-known work of Newton by F. N. David (*Annals of Science*, **13**, 1957, 137-147).

More than a third of the letters and papers in this volume are published for the first time, and many others have not previously appeared in full. There are three pages of Errata, Corrigenda and Addenda to volumes I and II, including an important correction to note 4 on page 12 of the former, where it was implied that Newton was an alchemist. It is now made clear that, whereas alchemists believed that matter could be transmuted by change of *qualities*, Newton's conception of transmutation implied instead a rearrangement of *particles*, as in ancient atomic theory.

The present magnificent volume, together with its two predecessors, is a credit to all concerned. The one small regret I have is that the history of individual letters and other papers is not indicated.

G. J. WHITROW

An Introduction to the Philosophy of Science. By Arthur Pap. Free Press, Glencoe, 1962. Pp. xiv + 444. $7.50.

THIS book was finished by Arthur Pap shortly before his death in September 1959, and it has apparently been published very much as he left it. I say 'very much' rather than 'just' because the bibliographies, which were originally compiled by Mr B. Berofsky, have been brought up to date and some one has modernised an argument on page 411 by introducing a reference to the international situation in 1961. At the beginning there are two short prefaces by Pap himself (one for general readers and one for instructors) which contain some interesting remarks about the demarcation of the philosophy of science and its place in our intellectual scheme. At the end there is a short memoir of Pap by Professor Brand Blanshard who knew him as a colleague at Yale.

Those who have read Pap's earlier works, in particular his *Elements of Analytic Philosophy*, his *Analytische Erkenntnistheorie*, and his *Semantics and Necessary Truth*, will expect to find here thorough, scholarly, sensible work, and they will not be disappointed. Although (as Professor Blanshard tells us) Pap reacted strongly as a young graduate against an earlier interest in

Hegel and retained until his death a tendency to use the word ' metaphysical ' as a term of abuse, equivalent roughly to ' beyond reasonable discussion ' (cf. pp. 321 and 323), he never became a party-line positivist. Like Carnap, from whom he learnt a lot, he was more anxious to find the truth and to set it out clearly than to strike an attitude and make a literary impression; but unlike Carnap, he could not swallow the conventionalist account of necessary truth, the operationalist account of theoretical concepts, the probabilistic account of ' qualified instance confirmation ', the extensionalist account of natural laws, or the physicalist account of psychology. Since he also rejected the later philosophy (or anti-philosophy) of Wittgenstein, he was a rather lonely figure within the large group of philosophers who are commonly called analysts; but he continued to work vigorously, though his academic career was not always happy, and when he died at the early age of 37 he had succeeded in producing a great deal of highly intelligent writing on the philosophy of science. In a later generation he displayed intellectual virtues somewhat like those of Broad.

The most striking feature of this book is its great range. In the first part, called ' Meaning and Verifiability ', Pap considers carefully the relation of the principle of verifiability to various developments in science. One of his most interesting contentions is that significance may be relative to theories, e.g. that a sentence of some such form as ' X travelled faster than light ' may seem meaningful at one time and yet prove to be not merely false but meaningless when considered later in the context of a new physical theory (p. 54). I should have liked to read more about this, since it seems to me to be a very important reflection. The second part, on Mathematics, Logic, and Experience, contains some very good sense on such topics as the logicist interpretation of arithmetic, Poincaré's conventionalist account of physical geometry, and the conditions of measurement. It would, of course, be impossible to deal here in an adequate way with all questions about the foundations of mathematics, and Pap does not even attempt to explain the troubles of the theory of sets or the various post-Fregean suggestions about the nature of mathematics. But he shows that the word ' analytic ' has been used in a wider and a stricter sense, and that if ' synthetic ' is used in a strict sense there is nothing at all absurd in talk of synthetic *a priori* truths. The third part, on Induction and Probability, gives sympathetic but critical accounts of the work of Keynes, Carnap, and Reichenbach, and presents an extremely comprehensive survey of the questions that have been debated under these heads during the past forty years. I am not satisfied by Pap's suggestion that we make an analytic statement when we say ' What has usually happened is, under the same relevant conditions, likely to happen again ' (p. 243), but no one can accuse him of trying to bounce the reader into accepting this simple story, since (with Mr Berofsky's help) he gives all the guidance we need for finding and studying rival theories. The fourth

part, on Causality and Laws of Nature, comes down in favour of a view of laws like that held by Braithwaite (p. 302), but apart from discussion of the general question about dispositions and counterfactuals it contains also a chapter on Determinism and Indeterminism with a fairly detailed attempt to explain away the paradox arising from the so-called reduction of wave-packets. His solution depends on a distinction between general and singular probability statements, a distinction he has made and elucidated in the previous part. Finally in his fifth part, called 'Explanation and Justification', Pap discusses such topics as teleology, emergence, interactionism, and value judgments.

Obviously in a book of 450 pages no one could say all there is to be said on all these subjects, and the reader may be at first inclined to suspect that Pap's work is no more than a snippetty survey which mentions a lot of things with an air of learning but treats of nothing seriously. This would be a mistake. Although he has introduced a great variety of topics, Pap has put in nothing without sober argument, and a student who works through the book carefully will learn not only how to find his way about in the philosophy of science but also how to argue with honest rigour. I am not sure, however, that it provides a suitable *introductory* course, whether for 'science majors' or for 'philosophy majors'. Admittedly I have had no experience in trying to take beginners through such a course, but I am inclined to think that many would find it bewildering and that for most it would be wiser to concentrate attention first on a narrower range of problems where philosophical discussion could be combined with some work on the earlier history of thought, studied if possible in a few great texts. This is an old-fashioned policy which Pap rejected; for he says in his second preface (p. viii) that 'the philosophy of science course presents a singular opportunity to clear the philosophy classroom of the atmosphere of venerable antiquity and historical erudition and to think about philosophical problems from scratch'. We all know the serious dangers of the traditional approach, but it seems to me that history of thought, conceived as an investigation of the way in which men have reached their present certainties and their present problems, is a very important civilising influence. Without it philosophy becomes dogmatism or modish posturing and science a bag of tricks. Furthermore, since it is obvious (and not really disputed, I think, by anyone) that the problems we call philosophical all have a linguistic element (i.e. are due in part at least to linguistic confusions), philosophical training must inevitably involve a lot of close attention to words, and it seems desirable that the words over which young people are made to pore should have some interest besides that of illustrating fallacies. The great merit of Pap's book is precisely that it reveals a kind of understanding which cannot be gained without hard work. What I suggest is only that beginners should be encouraged to work in greater depth on a shorter front. When

they have got a feeling for perspective and learnt that there are no problems without background assumptions, they can profitably follow his guidance in their reading and discussion of modern works over the whole field.

WILLIAM KNEALE

The Philosophy of Mathematics. By S. Körner.
Hutchinson, London, 1960. Pp. 198. 12s. 6d.

IN this small but comprehensive book Professor Körner discusses with considerable economy of means most of the philosophical issues raised by the mathematics of arithmetic and geometry. In this last phrase I declare myself a sort of semi-formalist, not far from being on Körner's side; for in his discussion of the great schools he is also advocating a view of his own. Nevertheless, though he has a point of view, Körner succeeds in making quite clear just what are the leading philosophical doctrines and what they require in the way of presuppositions to be convincing.

The book begins with a short discussion of the main views of philosophers before Frege, represented by Plato, Aristotle, Leibniz, and Kant. Each of these is chosen as either originating or exemplifying a certain line persistent in the philosophy of mathematics till now. The philosophy of mathematics as seen by Körner is concerned with three broad issues, the structure and function of pure mathematics, of applied mathematics, and the concept of infinity. Only the first and third of these have been studied seriously, the second being little considered or conflated with the first. It is one of Körner's aims to distinguish applied mathematics as a quite separate intellectual structure, related to pure mathematics but not a part of it.

I shall assume that readers know what doctrines the clan names logicism, formalism, and intuitionism imply. Against logicism Körner raises the following objection: the logicists have been unable to produce an hereditary characteristic, say 'analyticity', by presence or absence of which logical and non-logical propositions can be unambiguously distinguished. Without this we can hardly understand the phrase 'reduction to logic', and unless we understand this phrase we cannot tell whether a logicist programme has had any success. Further logicists require the notorious axiom of infinity which is generally held to be a general empirical hypothesis. Thus even if the logicist could give a clear account of analyticity the doctrine founders on the difficulty raised by the requirement of an empirical hypothesis in what should be a wholly analytical mathematical theory of infinity.

In discussing formalism Körner makes a nice point in bringing out the close relationship between Hilbert and Kant. In criticism he argues that the concentration upon inscriptions gives a tacit but clearly empirical cast to those mathematical statements important to formalists; however 'quietly',

as Körner puts it, this is done. Furthermore the formalist who should be dealing with symbol-types passes himself off, at least in philosophical discussion, as dealing with symbol-tokens, that is with physical objects. It is then an empirical question whether such and such a device is a formula. This becomes critical in connection with putative transfinite arithmetics. As Körner says (p. 112) 'The line . . . which divides finite arithmetic from transfinite is not the same line as divides empirical from non-empirical concepts'. Furthermore the formalists attempt to do without a formal logic, as expressed in Curry's phrase 'demonstration *ad oculos*', cannot be realised since they require the concept of 'correct construction', and that any construction is correct is not a perceptual characteristic of it.

Quite correctly Körner identifies the central philosophical doctrine of intuitionism as the self-evidence criterion of truth, and indeed as introspective self-evidence. Quite rightly he rejects this. But one can hardly accept his reasons for rejecting it. He argues that there can be conflicting reports of the same experiences; but how can we say two persons are living through the same experience (*a*) when it is introspection which reveals the experience, and (*b*) they give 'conflicting' reports? Surely the word 'conflicting' is tendentious here—if the reports differ then they are having different experiences.

The positive account of the philosophy of mathematics which Körner gives is expressed in his highly idiosyncratic doctrine of exact and inexact concepts. Perceptual predicates, the matter of applied mathematics, are said by Körner to be inexact, and by this he means that perceptual predicates are characterised by being such that there are always borderline cases where it is not determined whether we should assign them or not assign them to the subject. Rather than adopt the more ordinary view that in pure mathematics and applied mathematics we have different interpretations (the one into idealised and the other into actual units) of the same formula whose deduction from premises is a matter of following the rules, Körner seems to want to argue that since pure arithmetical and geometrical concepts are exact while those of the corresponding applied mathematics are inexact there are *sui generis* pure and applied mathematics. This view depends upon the belief that is central to Körner's own account, that there can be a logic of inexact concepts. This belief I hold to be mistaken on the grounds that the contrast between exactness and inexactness is not a contrast between predicates but between properties. It is leaves which are indiscriminable shades of green, the predicate '. . . is green' can be made subject to as exact rules as we wish and does not thereby lose its descriptive power.

These suggestions of Körner's are, I am sure, of sufficient interest for it to be hoped that they will receive wider discussion than is possible in a review.

There is one minor misprint on p. 52 where in lines 23 and 25 one should read 'for' for 'from'.

R. HARRÉ

A Threefold Cord: Philosophy, Science, Religion. By Viscount Samuel and Professor Herbert Dingle.
Geo. Allen and Unwin, London, 1961. Pp. 280. 25s.

It is difficult adequately to review this book in the space available not because it contains much that is new but rather because it attempts to cover so wide a field, all of which is of direct interest to scientific philosophers.

Dingle's philosophy, except when it concerns ethics and religion, is beautifully clear and probably already well-known to readers. He is anxious not to be labelled, but it will save time to describe his outlook as a 'philosophy of experience'. Samuel's position is much less clear: he brings up the old argument against idealism that the cosmos existed before the human race. Dingle properly points out that all our knowledge of the past of the cosmos nevertheless depends on our experience, but he does not want to be labelled 'an Idealist'.

Samuel is worried about action at a distance and quotes Newton and Einstein as having declared it unacceptable and deserving of scorn. 'Unthinkable', no doubt it is; but it is strange that Dingle seems also to regard this matter as logically settled. Samuel proposes an 'aether', without the property of forming a system of reference and so not troubled by the Michelson-Morley results. This aether should consist of two states of 'energy', quiescent and active, but the former 'can exhibit no phenomena'. This quiescent 'energy' serves several useful functions. Neither Samuel nor Dingle is happy about an expanding universe but the latter points out that if spectral shifts are caused by fatigue on long journeys, the lost energy must go somewhere, if conservation is to be preserved and the sink of quiescent energy is a possible repository. Samuel himself quotes what is perhaps the best comment on his theory, from the late Sir Henry Tizard: 'A theory of the aether expressed in words seems too vague to be of much use and I don't see how you would put it into symbols.' Yet Dingle and Samuel surprisingly seem to agree in criticising Bondi for saying of some hypothesis that one of the weightiest objections brought against it is 'that it has not proved possible, so far, to put it into mathematical theory'. Samuel clearly distrusts mathematical physicists and Dingle, though he has no quarrel with them as such, is disturbed by the attitude of many of them.

Another point of debate concerns the usefulness of the concept of potential energy, which Samuel questions. There is nothing that water at the top of a hill has, which is not possessed by water at the bottom. He complains that students are still taught that 'a brick . . . which someone has lifted onto a table becomes different from a similar brick left on the floor'. To demonstrate the fallacy here, Samuel Johnson would, no doubt, have put his foot below the edge of the table and shaken both bricks: Dingle

points out, gently and courteously the usefulness of the concept of potential energy to physicists.

Here one must mention a serious flaw in the otherwise scholarly tone of the debate. Dingle, as is well known, has been engaged for some years in controversies about Relativity; first, about 'space travel aging' and later concerning a defect which he claims to have found in Einstein's Special Theory. He complains that his views have not received a fair hearing and that those who disagree with him have never adequately explained why, nor refuted his arguments. He points out, justifiably, that majorities are not always right. So far so good; but, especially since Samuel shows no disagreement with him on these matters, it is a pity to drag so much of this controversy into the debate, and still more, to complain in referring to the views of a distinguished colleague, 'that this nonsense was published in the leading scientific journal'. (It must be added, in fairness, that the same colleague had already referred, in a letter to *Nature*, to 'the absurdity of Dingle's views'; but two blacks don't make a white.)

Samuel has little to say about ethics and religion but Dingle, surprisingly enough, seems to be not a little confused on the former subject. A and B both do what they honestly feel to be right, yet their actions conflict; so Dingle seems to conclude that there is no absolute right. It is like a football match, in which each side is 'right' to oppose the other, an analogy dangerously near to the attitude often taken by organised religious bodies to wars, and one which of course Dingle, as a Friend, cannot approve. Surely in ethics, as in Science, honest men differ without both being right. Some day it will be known which was more nearly 'right', Dingle or McCrea.

Religion gets a very small share of the book. Samuel and Dingle do not seem to differ greatly but both their viewpoints are so different from mine that I could not deal with this question in a short review. One point is strange to me, however. Dingle points out that science is built on its past but believes that religion is not. 'Christianity would remain what it is if all records of Judaism were lost. But it is not so in science. Every scientific paper contains references to earlier papers. . . .' Can anyone imagine a New Testament containing no references to the Old?

Finally, a minor editorial criticism. Each author writes a section to which the other replies at length. This necessitates either repeating large sections of the earlier text, or constantly referring back to earlier pages. It is useful to have the author's initials at the head of each page, but in editing future dialogues, would it not be possible, without undue waste of paper, to allocate the left hand pages to one author and the right hand pages to the other, when they are discussing the same topic?

G. W. Scott Blair

The Brain as a Computer. By F. H. George.
Pergamon Press, London. 1961. Pp. 413. 63s. or $9.50.

THE full impact of cybernetics on philosophy has yet to be felt. Though cybernetics started in the electrical workshop, and was for a time a piece of technology, it has an underlying philosophy, a way of looking at the world, that, while probably not new, is now so highly developed, and so strongly knit internally, that its place in philosophy will eventually have to be recognised. It is becoming too strong to be ignored.

Its peculiarity is that it expresses the behaviourist point of view, but with technical resources, and with an insight, unavailable fifty years ago. Watson, simply trying to do in psychology what other scientists took for granted, was ahead of his time. As soon as he left the simpler phenomena of tropisms and reflexes, he had to supplement his ideas with so many *ad hoc* suppositions that his attempt collapsed under its complexity or became mere speculation. Then came the big general purpose computer. At last Man could think with his hands while making and using the computer, instead of trying to solve all the problems by pure thought. With the machine (and similar networks) doing things for him to think about, the basic ideas of communication theory, memory, feedback, etc., became almost self-evident. (There was no reason, logically, why the Greeks should not have developed these ideas: they just lacked the material forms to suggest the ideas and to keep them disciplined.)

The chief characteristic, philosophically, of cybernetics today is its absolute objectivity. As, by definition, it studies that which is common to man and machine, it must either admit that the machine has a subjective aspect to be studied (a possibility the world does not yet take seriously), or it must study in man only those aspects that are as observable as those of the machine. So it treats feeling as a class of behaviours, it treats memory as a correlation between present events and past, and it treats cognition by studying what the organism, or some part of it, *does* about the stimulus. In a sense, the point of view of cybernetics is simply that of behaviourism; what is new is that behaviourism has returned to the scientific arena armed with methods and techniques immeasurably more powerful than those available to Watson.

Its impact on philosophy will probably be greatest in epistemology. 'How do I know?' is intimately related to the question 'How does information come to me?' So much is now known about the transfer of information, both quantitatively and qualitatively, and so many facts have been discovered that are quite contrary to naïve intuition, that any discussion of epistemology runs serious risks if the speakers are not well informed in communication theory.

For these reasons, Dr. George's book is likely to interest a wide circle of

philosophers as well as the psychologists for whom it is more particularly written. He says: 'Our main aim in this book is to show the part that cybernetics can play in helping to solve the traditional behavioural or psychological problems within the field of cognition.' It covers the field widely, for with cognition are considered the various other topics, such as learning, memory, and sensory physiology, that must be considered with it. It is written primarily for the psychologist of physiological leaning, who is concerned basically with behaviour at the observable or demonstrable level.

The book is easy to read, but the ease comes from the author's skill. In fact, the range is extensive and the treatment thorough. He is to be congratulated on having written an account which is complete and authoritative, yet everywhere able to keep the matter subservient to the flow of meaning. Personally, I would have preferred a little more on the fundamentals of information theory (neither equivocation nor redundancy are mentioned), but as the book runs to over 400 pages the request may be unreasonable. After this remark I can recommend the book unreservedly. As it has no peer, it must be regarded today as the standard text on the subject.

W. Ross Ashby

The Psychology of Thinking. By Robert Thomson.
Penguin Books, Harmondsworth, 1959. Pp. 215. 3s. 6d.

This book in the Pelican series of books on psychology is well up to the average of the series. It is carefully written, provides the reader with adequate references and bears evidence of being based on primary rather than secondary sources. The topics it deals with include the usual ones that come under this heading in textbooks of experimental psychology. It is however, more up-to-date than these, giving a detailed account of the recent work of Bruner and his associates on strategies used in attaining concepts, judged in the light of certain ideal strategies, as well as Piaget's recent work on the formation of logical and mathematical concepts in children. The author's intention here is admirable, but I am not sure how far he will have succeeded in communicating this rather difficult material to the interested layman.

The book is written very much from the point of view of learning-theory, a little uncritically so, to my mind. On the other hand, whilst the experimental work of Gestalt psychologists and those influenced by them is fairly reported, the author's understanding of Gestalt theory seems rather superficial. He shows his bias by including in a book on thinking a good deal on learning, but nothing on perception.

The author is a product of the new PPP degree at Oxford, his alternative subject having been philosophy. The influence of this is manifest in this

book; indeed, Professor Ryle has more page references in the Index than anyone else. Ryle's influence is evident in a much more catholic conception of what thinking is than is customary in psychology. The result of this extensive rather than intensive approach is a tendency to be prosaic at the expense of theoretical imagination, and to dwarf what psychology has done by frequent references to the variety of activities that can be called thinking, not yet the subject of scientific investigation. On the other hand, the author does not follow Ryle's beckoning and demand more taxonomy of psychology or criticise the experimental approach for its neglect of context. It is surprising, in particular, that, in the short chapter on Languages and Thought, he does not criticise the conception of language implicit in the two very rudimentary stimulus-response analyses of it by learning theorists that he describes. Altogether he is too loyal to his leaders, and the cutting edge of each discipline is dulled so that they do not hurt one another.

W. H. N. HOTOPF

The Rhetoric of Science, A Methodological Discussion of the Two-by-Two Table.
By Roy G. Francis.
Minnesota University Press; London: Oxford University Press, 1961.
Pp. vii + 183. 38s.

THE author makes an attempt to grapple with the considerable difficulties attendant upon the development of a rigorous statistical methodology for application in sociology. Instead of attempting to cover the whole area he has concentrated on the theory of the 2 × 2 table, though he uses this only as a thread on which to hang his discourse. This latter takes the form of a series of somewhat rhetorical assertions such as this on page 8:

> Whatever else he does . . . a scientist . . . comes to a decision about propositions. He must decide whether a statement is true and should be incorporated into science, or is false and must be rejected.

On the next page we find:

> A proposition is a statement about the real world. Logically . . . it may be true or false, but, to the scientist, a proposition may have another truth value—it may be held in doubt.

This is one example of many in which a questionable statement is made, only to be contradicted or qualified later on in the book. Such examples give one the impression that the author was thinking out what he had to say while writing the book rather than before sitting down to write. Such a tentative approach is probably inevitable in the area which the book tries

to cover but it is somewhat repugnant to those of us whose main preoccupations have been in the natural sciences where problems of methodology are relatively speaking so simple. This reviewer hopes that the author will be encouraged to develop his ideas in a sociological context but must report that for those to whom the use of probabilistic concepts is already familiar the work is likely to be found lacking in precision.

G. A. BARNARD

Unpublished Scientific Papers of Isaac Newton, a selection and translation by A. Rupert Hall and Marie Boas Hall. Cambridge University Press, 1962. Pp. xxi + 416. 60s.

THIS admirable latest addition to the recently expanding corpus of Newtonian publications is a selection of twenty papers from the Portsmouth Collection in the University Library, Cambridge. This collection was presented to the Library ninety years ago, so it would seem that scholars on the banks of the Cam have been unaffected by the general speeding-up of life in other parts of the world. Indeed, this is the first attempt to add to the published collection of Newton's scientific writings since 1838, when S. P. Rigaud printed a document entitled *Propositiones de Motu*. The editors are to be congratulated both on their initiative and on the skill with which they have performed their self-appointed task. The result must be judged by the highest standards of scholarship. It sets a worthy precedent for the publication of Newton's mathematical manuscripts of which, I understand, the first volume is now in the press.

In making their selection, the authors state that they have been chiefly interested in tracing Newton's ideas on the nature of matter, and that in these 'one penetrates as profoundly as may be into his comprehension of the universe, into the shadowy realm where physics, metaphysics and theology overlap in Newton's mind'. In addition, they also include some papers on other topics, notably mathematics. The translations from Latin are the author's own, but they acknowledge their debt to Andrew Motte as a good guide to the seventeenth-century meaning of a Latin word where such reference has been necessary. They point out that Newton's form of argument and thinking was influenced by the structure of Latin prose.

The present book is divided into the following parts, each prefaced by a useful introduction by the authors: Mathematics, Mechanics, Theory of Matter, Manuscripts relating to the *Principia*, Education, Notes. The last comprises Newton's annotations and comments on Hooke's *Micrographia*, which are presented as good examples of Newton's habit of making notes—often over-elaborate—on everything he read. (This habit he formed while still an undergraduate: he must always have read with a pen in his hand.)

From these notes it is clear, as the Halls remark, that he enjoyed catching Hooke out in a contradiction, long before he had either met him or had any personal dealings with him. In fairness to Hooke, however, it is clear that Newton approved of all his work in the *Micrographia*, except for his view on light and colour.

Of greatest interest to the student of Newton's philosophy of science is the manuscript ' De Gravitatione et Aequipondio Fluidorum '. The authors conclude, after a careful discussion, that the hand is certainly Newton's, although at first sight one might doubt this. The form of the hand is suggestive of Newton's youth, and the manuscript may have been written between 1664 and 1668. It contains many ideas which Newton developed more fully later and throws much light on their origin. Although the subject is ostensibly hydrostatics, he never gets really down to it but spends most of his time criticising the foundations of Descartes's natural philosophy. As the Halls remark, the work is worthless as a contribution to fluid mechanics but ' as revealing Newton's thought (early in his life) on many other topics it is of high interest '. In it we find Newton's concept of absolute empty space developed in conscious criticism of Cartesianism. Body he regarded as a part of space endowed with certain empirical properties. God he believed to be ubiquitous and eternal. In other words, space and time were not created with the universe but are necessary concomitants of God's existence. For example: *Nullum ens existet vel potest existere quod non aliquo modo ad spatium refertur* (No being exists or can exist which is not related to space in some way). Since extension is therefore uncreated, eternal, and uniform, it cannot be identified with body, as Descartes claimed, for in Newton's view body possesses none of these properties. Instead, it is a creation of the divine will. Newton argues that the mechanical philosophy of Descartes promotes atheism by assigning to matter the independence and *reality* which belong to God *alone*. As the Halls remark, Newton's argument comes close in places to the views expounded by Berkeley: ' strange though it may seem that Newton who is commonly regarded as furnishing the scientific substratum of Locke's philosophy should appear on the same side as Locke's critic.' But, whereas Berkeley invoked God, who perceives everything, as the cause of material existence, Newton maintained that matter is, strictly speaking, an illusion, so that he could thereby demonstrate the existence of God.

G. J. WHITROW

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ANNOUNCEMENT

A new philosophical Journal, *The American Philosophical Quarterly*, will begin publication in January 1964, edited by Professor Nicholas Rescher, University of Pittsburgh, Pittsburgh 13, Penn., U.S.A.